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THE PRESENT POSITION OF ANTISEPTIC SURGERY

(A reply to Mr. Tait's recent criticism of Sir Joseph Lister's Berlin Address).

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Read in the Post-Graduate Course of the University of Toronto, Dec. 20, 1890.

FOR several years it has been my custom in my lectures at the University of Pennsylvania to pass rapidly over the evidence in support of the antiseptic theory of wound treatment with the remark that the time for argument had passed, and it was merely necessary now to present such facts as might serve to make clear such principles involved and to emphasize the enormous benefit that has resulted from the application of those principles to practical surgery. It seemed to me that the theory of antisepsis rested upon a mass of correlated facts, experimental, clinical, and bacteriological, sufficient to establish it on a firm foundation, and that the only debatable ground remaining was that relating to the selection of methods and the improvement of details. I considered that the problem had been so scientifically stated, and its solution had been so thoroughly accomplished that, as regarded the general principles, no opposing views worthy of mention remained for discussion or refutation.

I also thought that the whole history of the development and establishment of this great theory, while including the admirable labors of hundreds of medical men in all parts of the world, and constituting one of the brightest chapters in the record of human progress, yet centered around the figure of the English-speaking surgeon whose name was identified with it from its incipiency, in whose mind the grand conception first took practical and definite shape, and whose patient, unassuming, conscientious work in elevating his original thought into a working hypothesis and in transforming the latter into a theory of world-wide acceptance, seemed to me to present perhaps the best, and certainly the most important, example of the application of purely scientific methods to practical surgery to be found in the history of the profession.

These, I say, have been my views, stated more and more dogmatically to successive classes as time went on, and with less and less thought of possible error or of contradiction from respectable surgical authorities. Of course there are always persons in every profession and in every community whose brain-cells react to new thoughts and ideas as their grosser tissues to foreign bodies. A condition of irritation is set up, followed by the expulsion of the intruder or by its encapsulation and virtual disappearance. Vaccination for variola has to-day its dozens of bitter opponents; for other infective diseases, its hundreds of skeptics; the theory of evolution, which has revolutionized the natural science of this century, is still violently attacked;

and although the laws of gravitation and of planetary motion are now quite generally admitted, we have in Virginia a colored clergyman who still proclaims his belief in the sun's motion. As regards the antiseptic theory, we can usually afford to turn a deaf ear to this class of opponents, many of whom have about as much claim to speak with authority on surgical or scientific topics as the Reverend Mr. Jasper on astronomical subjects.

When, however, a noted surgeon, a successful operator and a vigorous controversialist, undertakes to traverse the whole line of thought and argument upon which my confident acceptance of the theory was founded, denies its basal facts, ridicules its logic, jeers at its methods, challenges its records and abusively attacks its author and his supporters ; and when I find such views, so expressed, published and republished in the most reputable medical journals of the day, with little or no editorial censure, it seems to me proper that those of us who are teachers should once more review the evidence, consider the situation, and, according to our conclusions, publicly reaffirm or renounce our faith in the theory and practice of antisepsis.

On the 4th of last August, I had the pleasure of hearing Sir Joseph Lister deliver before the International Medical Congress at Berlin an address upon "The Present Position of Antiseptic Surgery." He evidently did not think it necessary in the presence of that vast audience, containing many of the most distinguished men in Europe and America, to defend or even to restate his position as to antisepsis, but devoted his time to noting the new light which had been thrown on the behavior of wounds by the results, on the one hand, of Koch's discovery of the method of cultivating microbes upon solid media and, on the other of Metchnikoff's researches into the phagocytic action of the migratory or amœboid cells of the human body. According to Lister, Koch's work has rendered it possible to study with greater precision than ever before the habits and behavior of micro-organisms, and he instanced the discovery of the cholera microbe as a notable example of the results of this method. Detailing some of Metchnikoff's experiments to prove the antibacteric action of normal leucocytes, he called attention to the explanation it offered of much that was hitherto mysterious in the relation of micro-organisms to wounds ; the healing, for instance, of wounds like that made in the operation for hare-lip, the posterior edge of which is perpetually bathed in saliva containing septic bacteria. The destruction of these microbes by the leucocytes which people the lymph at the edge of the wound satisfactorily explains the rapid healing which we uniformly obtain after this operation. So, too, he thought, that in cases where fine silk ligatures are used unpurified and left in closed wounds, the phagocytic action of the normal tissues may destroy the microbes that have gained access to the interstices of the thread, and prevent their fermentative or putrefactive action on the discharges.

He then considered the question of drainage and of irrigation, pointing out the possibility of dispensing with both in many wounds, and suggesting that in the new light thrown upon the ability of normal tissues to protect themselves, contamination from atmospheric organisms may perhaps also be disregarded, provided no septic matter be otherwise introduced into wounds. In support of this, he noted the fact that it had been found that the free entrance of air containing microbes into the pleural cavity in cases of empyema had produced no harmful results in the days when the spray was irrationally depended upon to sterilize such air, and instanced the transformation of the purulent contents of the pleural cavity into a rapidly diminishing serous effusion, the closure of the external opening, the resumption of the normal functions of the pleura, the expansion of the contracted lung through atmospheric pressure,*

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*This statement Mr. Tait criticises (if it can be called criticism) most violently, calling on the "shades of Newton and Toricelli in mock dismay at the disregard of physical laws which he thinks it manifests. He misquotes Lister, who spoke of the closure of "the external opening." This Tait transforms into "closure of the affected cavity," and then says that "given a membrane to which the atmosphere has free access on both sides, on both of these sides the pressure of the atmosphere will be exactly the same"—a self-evident proposition, but one which has nothing to do with the condition of the lung and pleura after the chest wall has become impervious. He says, also, that when the closure of the pleural cavity is completed "the natural dimensions of the affected lung are *always* remarkably diminished." In contradiction of his whole position in this matter, I would refer to the following authorities: West, on Pneumothorax (*The Lancet*, 1887, vol. 2, p. 353); Williams, on the use of a Valvular Tube in Empyema (*British Medical Journal*, May 18, 1889); Reynolds, on Pneumothorax Consecutive to Emphysema (*Manchester Medical Chronicle*, October, 1889); Douglass Powell, on Variations in Intra-thoracic Pressure (*Transactions of International Medical Congress*, 1881, vol. 2); Donaldson, on Diseases of the Pleura (*American System of Medicine*, vol. 3, pp. 522, 559). The latter author says : "The dilatation of the lung is produced by the disappearance of the intra-pleural pressure and the pressure in the opposite direction from the bronetc., as a beautiful example of the reparative processes of nature when uninterfered with by mischievous agents from extrinsic sources. The contrasting course of those cases, in which, in pre-antiseptic days, the discharges escaping from the wound became infected and underwent putrefactive fermentation, is familiar to most of you.

He then emphasized the need for antiseptic rather than aseptic dressings in cases where large discharge is unavoidable, and concluded with a reference to the double-cyanide dressing which he has been using for eighteen months.

This address seemed to me as I listened to it to be another striking example of Lister's remarkable willingness to receive and profit by all new discoveries and all genuine advances bearing upon the antiseptic theory. So far as I know, he has never remained silent in the face of satisfactory demonstration that any portion of his method was unnecessary or illogical. As he gave up the spray when it became evident that it was not accomplishing its work, as he has from time to time discarded various antiseptics in the search for the ideal one combining permanency and certainty of action with absence of irritating qualities, so he now is willing to minimize the dangers of atmospheric contamination and to discard washing, irrigation, and even drainage in appropriate cases, although for years he has been conscientiously emphasizing their importance. Surely this is the true scientific spirit, as rare as it is admirable, and an additional evidence of the single-mindedness and absolute fairness of this great investigator.

On the 27th of last September there appeared in *The British Medical Journal* an article by Mr. Lawson Tait, consisting of an address delivered a short time previously, and entitled "The Present Aspect of Antiseptic Surgery: A Criticism of Sir Joseph Lister's Address at the International Congress."

Of the tone, taste and temper of this essay, I shall have but little to say. It would be difficult to characterize it properly and preserve the dignity and decorum which should belong to scientific discussion, but which are so conspicuously absent in Mr. Tait's paper. So far, however, as concerns our present purpose, it may be considered from two standpoints: 1st. As it denies the truth of the principles underlying the practice of antisepsis, and advances an alternative theory applicable to the treatment of wounds. 2nd. As it attacks the prevailing antiseptic methods.

1. THE PRINCIPLES INVOLVED. Mr. Tait draws an elaborate comparison between the phlogiston theory of Stahl and the antiseptic theory, asserting, to use his own words, that we have a perfect parallel to the former in "the septic theory of inflammation and fever which is the favorite hobby-horse of our own day." He adds, "everything at present has a septic origin and a septic inception, yet I venture to say that before the present generation has run out the word antiseptic will be all that is left to represent the strange structure, just as antiphlogistic was the only word left to represent the phlogistic theory in the middle of the present century." He continues by asserting a want of logic in the use of the term "theory" at all, saying that "instead of the septic or antiseptic fact, Lister and his still more illogical disciples talk of the septic or antiseptic theory, whereas there is no theory about it at all, but an absolute and ludicrous logical error." He then opens his argument by denying that the cholera bacillus has been definitely isolated or that it can be cultivated with certainty and precision; and says that even if it has and if it is potent for production or reproduction, the fact that if a thousand people drink the same germ-infected water only a hundred or so will be affected, and that the majority of these will recover, shows that the facts about germs in the human body do not coincide with the facts of the germs in the gelatine flasks, and that, therefore, they cannot stand as the basis of a working hypothesis, far less of a theory.

It is difficult to follow the vagaries of this extraordinary paper; but if all this means anything whatever, it means, taking the cholera bacillus as a type, all deductions based upon bacteriological investigation are denied because the growth and reproduction of micro-organisms in the body are so influenced and altered by physiological and vital processes as to run a course somewhat different from that which they take in flasks or test-tubes. For the same gen-

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chial surface." The whole question is, of course, a distinct digression, but is used by Tait in justification of his assertion that Lister 'possesses crude notions of logical definition "!!

eral reason and because it will not immediately explain the difference among the clinical courses of various infective diseases, Mr. Tait rejects the phagocyte theory, apparently assuming that if it is true there should be a uniform destruction of bacilli of all varieties, one might as well deny the essential element of conception, the fusion of the ovum and sperm cell —possibly an instance of phagocytosis—because of the variations in the resulting animal. This position is, however, in harmony with his denial of the value of vivisection for similar reasons, and illustrates the working of a mind which he is pleased to consider "logical." The assumption that this is what he is clumsily attempting to say is also justified by several other paragraphs, which will serve at once to display his real ignorance of the teachings of Lister and to illustrate the character of his address. He says, "If the entrance of germs into a wound was the immediate and real cause of suppuration and of consequent poisoning, the device of the spray was the most completely logical appliance that could have occurred to the mind of man." "Even if the phagocytes are the means by which the tissues resist the omnipotent and ever present germs, it is the phagocytes and their conditions which must constitute the really important elements of the question; keep your phagocytes up to the mark and you need never bother about germs." "With the Listerian, one germ is as good as a thousand." "If it would gratify the phagocytes in any way, I would stuff the abdomen like a pudding with the germs or bacilli of decomposition, provided there was nothing present for them to feed upon" He finally, after more vituperation and self-glorification, sums up his position in the two following paragraphs: Lister's view was, "Keep out the germs and you may leave blood-clot (and other matters) to take care of themselves." My view was and is, "Get out all decomposable matter and you can let the germs in freely." "There are two factors in the trouble, and it can be shown conclusively that one, the germs, are wholly inconsiderable without pabulum on which to feed; whilst the other, the pabulum, is sure to breed trouble because it is practically and mechanically impossible to keep the germs out; they exist already in the blood and elsewhere, and are ever present, according to the best authorities."

This amount of quotation I was reluctantly compelled to make to elucidate his views, but we may now ask, "What are the *facts* in the case?"

The evidence necessary to prove the definite etiological relation of a particular micro-organism to a specific disease in man is as follows: The organism must be unmistakably recognizable at different periods of its growth; it must be isolable; it must be capable of cultivation alone and free from association with other organisms; it must be invariably associated with the disease in question; it must be capable by inoculation of producing that disease; it must retain this power through an indefinite series of cultures. If these conditions were fulfilled in but a single instance, it would be sufficient to demonstrate the folly of the general attack on all germ theories contained in Tait's paper; but at least five well-known examples may be mentioned of indisputable relation of cause and effect between specific pathogenic microbes and infective disease. Since 1879, when Koch first demonstrated the connection between definite micro-organisms and various wound diseases, the evidence has steadily accumulated. Ogston in 1881, Rosenbach and Cheyne in 1884, Passet in 1885, Zuckermann in 1887, have shown that the various staphylococci have as distinct a causative relation to acute suppuration as has the yeast plant to the alcoholic fermentation. They can be found in and cultivated from the pus in every recent abscess; they invariably produce suppuration when pure cultures are injected, even to the twentieth generation. Their effect may or may not be aided by the ptomaines which they produce, but as the amount of ptomaines is directly proportionate to the number of microbes present, and as the ptomaines are never produced at all in the absence of the microbes, the question as to the relative importance of the two has, as Senn long ago said, but little interest for the practical surgeon, and can have none at all for the "purely unscientific person" that Mr. Tait with truthfulness represents himself to be.

Another example is to be found in malignant pustule, the specific cause of which, the bacillus anthracis, is readily cultivated in different media and produces the disease when inoculated in the smallest possible quantity.

Fehleisen in 1883 showed that erysipelas depended upon a streptococcus, inoculation of which when quite pure produced the disease in six out of seven cases, the exception occurring in a person who had recently had an attack of erysipelas and was scarcely more than convalescent.

Acute osteo-myelitis has until recently been thought to be due to the ordinary microbes of suppuration, but the researches of Becker and Krause (1883) and others have made it probable that a specific micrococcus is the essential element of the disease.

Tubercle furnishes the last of our examples of microbe disease, and we have just learned that to Koch, who in 1882 discovered the tubercle bacillus, we possibly owe the equally brilliant and infinitely more important discovery of its antidote.

In the face of this collection of facts, of such transcendent importance to surgery and to the world at large, we can afford to pass by without further mention the bacilli of tetanus, typhoid fever, and glanders, the micrococci of lobar pneumonia and of gonorrhœa, and the microbes of cholera and of gangrene, although it is quite clear to all persons capable of weighing scientific evidence that it is a question of time only when our knowledge of these microbes shall be as definite as of those I have enumerated.

In the face of this evidence, we may assume that Tait's general and contemptuous rejection of *all* surgical theories dependent upon our knowledge of germs is not warranted by the facts in so far as suppuration, erysipelas, tubercle, anthrax, and osteo-myelitis are concerned, and therefore that there is no *a priori* ground for rejecting *every* theory involving a belief in the potency for mischief of micro-organisms.

It is more directly to the point, however, to inquire into the evidence in favor of the germ theory of septic wound disease, and here we must briefly consider some very elementary questions. Mr. Tait takes great pains in one portion of his paper to emphasize the fact that "such bacilli as cause decomposition and such as have specific properties split out from dead organic matter some horrid things," *i.e.* in other words, that they act as ferments, causing that form of fermentation which we know as putrefaction. Mr. Tait is as usual inaccurate in his statement—as the *micrococcus erysipelatosus*, for example, which is undoubtedly a microbe with specific properties, does not cause any putrid change: but he would have been correct in asserting that all organisms produce changes in the materials in which they grow, which means that they all cause *some* kind of fermentation.

The antiseptic treatment of wounds might, under Mr. Tait's ruling, be described as a treatment directed against the causes of putrefaction in wounds: but a more accurate definition expands it, as Mr. Cheyne has suggested. so as to include treatment directed against the cause, not merely of the putrefactive fermentation, but of all fermentations. Accepting this very liberal definition, our scientific evidence leads us to the adoption of the antiseptic theory by the following steps, each of which is as demonstrable as the single rule of three or the *pons asinorum:*

Fermentation (whether putrefactive or otherwise) in organic substances, such as blood clot or serum or pus, or the discharges from wounds, depends upon the access to these substances of micro-organisms. The proofs of this fact are twofold. Experimentally, it is shown by the certainty with which easily decomposable substances like blood, urine, or beef tea, may be kept indefinitely sweet by simply protecting them from germ contamination. Clinically, it forms the basis of all subcutaneous surgery; explains the difference between simple and compound fractures; and, taken in conjunction with another fact, viz., that the fluids and tissues of the healthy living body are practically sterile, it also explains the difference between the behavior of a case of arterial occlusion affecting a portion of the surface of the body and producing gangrene or sloughing, and a case of similar occlusion affecting an internal organ and producing atrophy and retrograde metamorphosis.

The last fact I mentioned—the sterility of the bood and tissues—is denied by Mr. Tait, who says "the germs exist already in the blood and elsewhere, and are ever present, according to the best authorities." The elaborate and carefully conducted experiments of Hauser,[*] Watson Cheyne[†] and others completely contradict this statement, which is really the foundation of Mr. Tait's argument. As we have seen, the evidence that germs cause fermentation is undeniable, and it is equally true that germs can be excluded from wounds by antiseptic precautions, or can with less certainty be destroyed after they have gained access. This statement rests, on the one hand, upon microscopical evidence (microbes being demonstrably absent from the discharges of properly sterilized wounds and as constantly present in suppurating or sloughing wounds); and, on the other hand, upon a mass of clinical testimony familiar to every member of the profession. A portion of this testimony, in spite of its triteness, it is necessary to recapitulate in order to complete the argument: The germ theory of fermentation is undisputed; fermentation in wounds can therefore be prevented by the exclusion of germs; it has been shown microscopically that these can be kept out (asepsis) or can be destroyed (antisepsis); practically, therefore, it only remains to show the effect upon patients, with operative or other wounds, of excluding or destroying germs to show the value or lack of value of antisepsis and the antiseptic theory. By looking back a few years to the very beginning of the employment of antiseptics, we can obtain the most striking and convincing evidence of the effect of treatment directed almost exclusively, though then very imperfectly, against the introduction of bacteria.

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[*] Archiv. f. Experiment: Pathologie and Pharmacol: bd. 20 p. 1

[†] Brit. Med. Jour., March 3, 10. 1888.

The record of the work of Prof. Lister may well begin our series of examples. In Glasgow, in 1864, 1865, and 1866, Mr. Lister's mortality in a series of operations of all sorts was 45.7%, largely from septic diseases. About this time he began to employ, gradually, some antiseptic methods in his treatment of wounds and during operations. In 1867, 1868, and 1869, his mortality fell to 15%. At Edinburgh, having greatly improved the details of his system, we find that from 1871 to 1887 he treated 553 grave surgical cases, with a mortality from septic disease of only .36%, a diminution in the death rate which, when we remember that these different results were obtained by the same man operating upon the same class of patients, and for the same injuries or diseases, is so striking as to be in itself conclusive. Still later, an opportunity was afforded to compare Mr. Lister's results with those of a colleague, Mr. Spence, working in the same hospital, but declining to employ antiseptic methods. The total results of their major operations showed that Mr. Spence lost just about three patients where Mr. Lister lost one, while the deaths from infective diseases were 2.4% among Spence's cases, and one-third of 1%, or eight times less, among those of Mr. Lister. When we turn to the work of other surgeons, we find evidence, if possible, still more conclusive, of the value of these methods. Nussbaum has shown that during forty years in his clinic, under his own direction as well as that of his predecessors, among whom was Stromeyer, the deaths from wound diseases were so common that patients with even the slightest injuries often succumbed to them; that erysipelas and abscesses were matters of daily occurrence; that 80% of all wounds and sores were attacked with hospital gangrene, and that nearly all patients with compound fractures died; and he states that immediately upon the introduction of the antiseptic system, all these diseases vanished, and healing by first intention, previously almost unheard of in his service, became the rule instead of the exception. Prof. Volkmann, in an address before the International Medical Congress, which met in London in 1881, testified strongly and clearly to the results obtained in his own practice, selecting two subjects especially, compound fractures and major amputations, as evidence of the value in his hands of the antiseptic method. He said that the mortality after compound fracture had, during the long labors of the surgeons who preceded him, as well as during his own, reached the sad height of 40%. Immediately before he adopted the antiseptic method of treating wounds, his last twelve patients with compound fracture had all died of pyæmia or septicæmia. From that time up to the period at which he delivered this address, he had treated 135 compound fractures without losing a single patient from either of these wound diseases; 133 were cured; two died, one of fatty embolism of the lungs during the first few hours, and one, a drunkard, of delirium tremens. As to the amputations, he asserted that he now cured each year, with the antiseptic method, more cases of amputation of the thigh, for example, than during all the rest of his labors before the introduction of the method. In an article on the treatment of compound fractures, Prof. Dennis, of New York, has compiled further evidence in this same direction. In the Pennsylvania Hospital, between the years 1839 and 1851, there were treated 116 cases of compound fracture of the leg and thigh; excluding those cases requiring amputation, there were 51 deaths, a mortality of 45%. In the New York Hospital, during the same period, there were treated 126 cases of compound fracture of the leg and thigh; excluding amputations, there were 61 deaths, a mortality of 48%. From 1860 to 1876 there were reported from the surgical clinics of Vienna and Zurich, by Billroth, 180 cases of compound fractures; excluding amputations, there was a mortality of 41% from septic disease. In the Obuchow Hospital Reports of St. Petersburg, 106 cases of compound fracture gave a mortality of 68%. In Guy's Hospital, from 1841 to 1861, there were reported 208 cases of compound fracture, with 50 deaths, a mortality of 26%. After the introduction of antisepsis, this death rate immediately fell to 4% from an average of 40 to 50%; and in this article of Dr. Dennis, in which he reports 516 cases of compound fractures, there was no record of death from septic trouble in any fracture of the extremities, which was the class of injuries included in the above statements. I might easily multiply such evidence as this a thousand-fold, but it seems unnecessary to repeat what has long been so familiar. And yet it is upon this evidence, which I have endeavored to condense, that the antiseptic theory rests. The facts and figures could easily be replaced by newer statistical matter, but I have selected these for the very reason that they show better than more recent experiences the wonderful changes wrought solely by the employment of antiseptics under circumstances otherwise unaltered—drainage having been previously used, the amount of "pabulum" in the wounds remaining the same, the operators and surroundings being just as in the days of gangrene and pyæmia. Mr. Tait endeavors to break the chain of reasoning by saying, (1) That the germs are everywhere and cannot be got rid of—a misstatement; and (2) that the presence or absence of sepsis depends on the presence or absence of "pabulum," *i.e.*, dead organic matter, blood clot, serum, etc. It has been one of the axioms of antiseptic surgery from the beginning that scrupulous attention should be paid to hæmostasis and to drainage. No one has taught this more earnestly than Lister and his followers, and Tait's adoption of it as *his* "view" is evidence of his ignorance of the work of others. To be sure he says that Kœberle first taught him drainage in 1875, and gives due and deserved credit to Chassaignac for his work in this direction, but English and American surgeons have employed drainage fully and carefully since the early part of this century, and Syme (afterwards Lister's father-in-law), in 1826, made the importance of providing a free escape for the discharges from wounds the subject of one of his most important papers. It is easy to say that if you have nothing to decompose there will be no decomposition, for that is what it amounts to. "Get out all decomposable matter," he says, "and you can let the germs in freely," but in practical surgery this is by no means always possible. It is really to be regretted that Mr. Tait has not had an opportunity to see some general surgery, and particularly to follow Lister's practice at King's College Hospital. I am curious to know how he would interpret a case which I saw there in 1888, and described as follows in a letter to *The Medical News*. It was a case of operation upon a fractured patella, with elongation of the quadriceps by Cameron's method, in which there had been muscular spasm, followed by extensive oozing from the cut surfaces: "A large clot formed, producing considerable tension, necessitating the removal of the stitches, and causing at two points a slight separation of the edges of the wound. I watched this for ten days, and instead of the flush, the suppuration, the elevation of temperature and the alarming symptoms which would usually follow such hemorrhage into and about a great joint, the patient and the limb both remained perfectly passive, with absolutely no sign of either local or constitutional irritation, while the clot itself gradually contracted and the knee resumed its normal outline. During all this time there was literally not one drop of pus, and not a trace of offensive odor. A severer test of Prof. Lister's antiseptic methods could hardly be devised or imagined."

There was no lack of "pabulum" here, but it gave rise to no evil effects, while, on the other hand, I am sure that there is not a practical surgeon in the world to-day, no matter how "humble" or "unscientific," who doesn't know what the result would have been if the germs had been "let in freely." The numerous successful cases of Schede's method of utilizing blood-clot in the filling of bone cavities are similar arguments and of a most convincing kind.

Ignorance of general surgery and highly specialized experience offer the only explanation of such astounding statements.

Mr. Tait deals habitually with a membrane, the peritoneum, which has a remarkable power of self-protection. Microbes gaining access to the abdominal cavity are exposed to destruction either by the serum which it copiously exudes, or by a possible phagocytic action of its enormous numbers of endothelial cells, or by both. I have thought, too, that possibly the extraordinary cases of cure of peritoneal tuberculosis after simple laparotomy and flushing of the abdominal cavity were due to the stimulation of the membrane (through removal of pressure or through contact with the irrigating fluid) to greater exudation and of its cells to greater anti-bacteric activity. It is merely a supposition, but seems as reasonable as any of the explanations I have heard offered. At any rate these properties are well known, and if one could anywhere in the body depend on the restriction of the amount of "pabulum" rather than on the exclusion of the germs it would be here.

Mr. Tait, after contemptuously disregarding *all* surgical theories, including that of antisepsis, ventures upon one of his own. He thinks the phenomena of sepsis due to the "dose" of dead tissue or "pabulum," and of the poison resulting from its decomposition, and with a momentary forgetfulness of his "humility" he actually advances this as an original "working hypothesis." It would sound very much to the general surgeon as if Mr. Tait had never heard of sapræmia, which is described by a "Listerian" as follows: "Sapræmia or putrid intoxication represents that form of septicæmia in which a preformed toxic agent is injected into the circulation and in which the maximum symptoms are reached as soon as the poison has become mixed with the blood. This form of sepsis may be caused by any microbe, otherwise harmless, or with only slight pathogenic properties, as the bacillus of putrefaction, which causes putrefaction in any dead tissue, as, for instance, a blood clot or contused tissue; and the symptoms arise as the ptomaines are absorbed, and are appropriate to the amount absorbed, and subside with the cessation of absorption and their elimination through some of the excretory organs."

Mr. Tait's remarks against dosage are further evidences of his ignorance of Listerian work and teachings. He says: "With the Listerians, one germ is as good as a thousand." I suppose it is safe to class Mr. Watson Cheyne, whom I have just quoted, as a "Listerian." Indeed, he has for years been Lister's assistant and authorized mouth-piece. In Mr. Cheyne's lectures on "Suppuration and Septic Diseases,"* he not only asserted but proved experimentally that the number of bacteria introduced modified greatly the intensity of the symptoms and even the character of the disease. He experimented with cultivations of Hauser's *proteus vulgaris*, finding that one-tenth of a cubic centimetre caused immediate death; one-fortieth of a cubic centimetre caused death in six or eight weeks; smaller doses produced no effect. Similar observations have been made by Passet,† Pawlousky,‡ and others, and are as well known as any in connection with antiseptic surgery.

It is evident that what is original in Mr. Tait's "hypothesis" is not true, and that what *is* true is not original.

2. THE PREVAILING ANTISEPTIC METHODS.—It seems apparent at any rate that, to put it mildly, Mr. Tait is not a safe or reliable authority on the general subject of surgical antisepsis, but towards the end of this paper he ventures on a specific fling at Lister's last surgical dressing. After asserting that "Listerism" is "as dead as Julius Cæsar," he adds: "The mischief probably is not at an end, for we hear that the last phase of this astonishing craze is that wounds are to be dressed with still a new contrivance, one as deadly and dangerous as any that can well be imagined—a double cyanide." I am especially interested in replying to this statement for the reason that, owing to the kindness of Sir Joseph Lister (who during the summer of 1889 gave me confidentially the various formulæ which he was using, and supplied me with the materials from his own manufacturers of gauze

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*Brit. Med. Jour., March, 1888.

†Monatshefte f. Prakt, Dermatol., B. 6, No. 10, 1887.

‡Centralblatt f. Chirurgie, No. 48, 1887.

and chemicals), I was the first surgeon in this country to employ the dressing in question, and because I now have a series of cases in which it has been used, large enough to justify publication. The history of the steps which led up to the employment of this material must be briefly mentioned, both to explain its selection and in reply to the charge of "perpetual shifting."

The aim of the surgeon after every operative wound is, of course, to secure union by first intention. Any persistent irritation is a source of trouble in wounds, and the different antiseptics which have been employed have not been free from the the charge of contributing to the occurrence of excessive inflammation by their own irritant action. The typical antiseptic must, therefore, be as nearly as possible devoid of irritating properties. It must be germicidal—that is, it must possess the power of destroying the micro-organisms. It must have an inhibitory power—that is, it must prevent the development of such organisms. It must be stable—that is, it must not disappear from the dressings after they are made, or after they are applied to the wound, either by solution in the discharges or by volatilization.

In the old Listerian method, carbolic acid was the antiseptic employed; but this had the disadvantage of volatility as well as a great slowness of action as a germicide. Corrosive sublimate, which succeeded it, was stable and acted rapidly, but was exceedingly irritating, and, in addition, was precipitated by the albumen contained in the serum of the blood. This precipitate, it was discovered by Sir Joseph Lister, possessed powerful antiseptic properties, with much less power of producing irritation; and he therefore devised a form of antiseptic dressing called "the sero-sublimate gauze," which consisted of gauze charged with a solution of corrosive sublimate in the serum of the blood. This, however, was difficult to manufacture, and produced a harsh and non-absorbent material which was mechanically objectionable. It was succeeded, in his hands, by a combination of chloride of ammonium and bichloride of mercury, known as sal-alembroth, which, while much less irritating, was so exceedingly soluble in the blood serum that whenever the discharges from the wounds were copious, it was washed out of the dressing, leaving them without antiseptic property. For these reasons, Lister, in time, discarded this material, and employed for a considerable period a gauze containing 3 or 4 %, by weight, of the biniodide of mercury. This was less soluble, non-volatile, powerfully antiseptic, but, again, extremely irritating, so that the least contact with the skin produced an intense erythema, even going on to vesication.

Lister was then led to look further for the ideal antiseptic, and finally to employ the double or mixed cyanide of mercury and zinc which he is now using. Its most evident advantages are, first, that it is non-volatile; next, that is almost entirely unirritating; thirdly, that it is insoluble in water and only soluble in 3000 parts of blood serum; and finally, that, while it possesses but little germicidal value, its inhibiting power is so high that a 1-200 solution is sufficient to keep animal fluids permanently free from putrefaction. This combination of qualities is possessed by no other of the antiseptics which have been mentioned, and its deficiency in germicidal power is easily remedied by including in the manufacture of the gauze dressings which are impregnated with this material a small percentage (1 in 4000) of sublimate, enough to be germicidal, but too weak to be markedly irritating. As I have said, I was told the details of this method *confidentially* in the summer of 1889. The communication was confidential because, while Lister was experimenting with other dressings, notably the sal-alembroth, the announcement had been made by others that he was using them and they were extensively employed at a time when he was quite unprepared for publication upon the subject. Until November, 1889, therefore, I employed the double cyanide only in my private work, but since that date, when Lister published his own conclusions on the subject, have also used it after nearly all my operations at the University Hospital. I present herewith in tabulated form the results of the most important of these operations, exclusive of dozens of minor cases which, while as a matter of fact they often test severely the efficacy of any surgical dressing, might be expected to get well under almost any form of treatment.

I have grouped the cases roughly and have avoided unnecessary detail for the sake of economy of space, the main object in publishing them now being to establish the efficiency of the double cyanide dressing and its freedom from dangerous qualities.

TABLE OF SURGICAL CASES TREATED SINCE BEGINNING THE USE OF THE DOUBLE CYANIDE GAUZE.

(SEPTEMBER, 1889, TO DECEMBER, 1890).

Group.	*No. of Cases.*	*OPERATIONS.*	*MEMORANDA.*	*RESULTS.*
1	13	Removal of cervical glands for tuberculous adenitis.	Wounds varied in length from two to six inches; in five cases, the dissection was most extensive, being carried along the œsophageal line; in four, caseation and suppuration already existed.	Except in the suppurating cases, union by first intention along the whole length of the wound. Drainage tube usually employed, but withdrawn during first two or three days. Fever rarely 100° after second day. No deaths.
2		Trephining of cranium.		
	3	For compound fracture.	Wounds already infected by vulnerating body, and by dirty fingers; careful sterilization by 1:500 sublimate.	Union of scalp wounds by first intention, no pus, no fever, no deaths.
	4	For epilepsy.	In every case operation performed on account of history of traumatism.	In three cases union by first intention; one death from suppression of urine and uræmia in an old alcoholic.
3		Abdominal section.		
	8	For removal of uterine appendages.	In every case 1:10000 bichloride solution was used for hands and sponges, and when required, for irrigation. Tube used in three cases.	Union by first intention, except in two of the tube cases, in which stitch abscess occurred; no deaths.
	4	For intestinal obstruction.	In two cases there was general purulent peritonitis and great distention. The obstruction in all four cases was from bands.	In two, union by first intention, and recovery; in the others, death within first 24 hours.
	4	For peri-typhlitic abscess.	In two, removal of vermiform appendix; in two, evacuation of abscess and use of drainage tube.	One death in 24 hours, probably from ignorance of nurse, who neglected the drainage tube. In others, rapid recovery, without fever or other alarming symptoms.
	1	For removal of tumor of abdominal wall, and cure of ventral hernia.	Operation very tedious and prolonged, involving much handling of tissues. Wound brought together by sutures *en étages*. Patient old and infirm.	Extensive "aseptic suppuration," *i.e.*, without fever, pain, or other symptoms, thought to be due to traumatism to the thick layer of subcutaneous fat. Recovery with good, firm cicatrix.
4		Hypogastric section and cystotomy.		
	1	For exploration.	No satisfactory explanation found for violent and persistent vesical symptoms, which had lasted for 29 years.	Rapid healing, no suppuration.
	4	For removal of calculus.	Three stones weighing over two ounces. One small oxalate calculus.	All recovered without suppuration.
	1	For prostatectomy.	Very extensive operation, removal of large portions of the prostate.	Death on eighth day from uræmia and suppression of urine. Kidneys almost completely disorganized.
	1	For exploration and drainage.	An enormous prostate with absolute retention. Patient old and feeble.	Lived a year in comparative comfort.
5	2	Excision of varicose long saphenous vein.	Veins extremely long and tortuous. Patient disabled. Incisions 18 and 19½ inches in length.	Rapid union. No suppuration.
6	14	Excision of breast, with removal of axillary glands.	In all, complete removal of breast, opening of axilla, and removal of axillary glands.	Not a single suppurative case. Average time of healing and discharge from hospital, 11 days. Average number of full antiseptic dressings, three.
7	3	Arthrectomy of knee.	In all of them, most extensive removal of synovial membrane, ligaments, cartilages, and osseous foci of disease. All tubercular.	In all, rapid primary union of skin wound. In two, persistent suppuration in track of drainage tube, but final healing. In one at this date (eight months after operation) reopening of sinus. In others, apparent cure.
8	1	Removal of foreign body from knee-joint.	A portion of needle found partly within, partly without synovial cavity.	Primary union under first dressing.
9	3	Removal of superior maxilla.	All three cases of malignant tumors.	Primary union of skin incision; all recovered from the operation.
10	1	Removal of both superior maxillæ, with a portion of the malar bone.	For osteoma of both maxilla.	Primary union of skin. Entire recovery from the operation.
11	2	Removal of a portion of the inferior maxilla.	For osteo-sarcoma.	Primary union of skin. Prompt recovery.
12	2	Excision of elbow.		
	1	For tuberculous arthritis.	Patient had already undergone several operations upon other joints.	Union by first intention under two dressings, except in track of drainage tube. Entire union in three weeks. All movements of elbow preserved

TABLE—*Continued.*

Group.	*No. of Cases.*	*OPERATIONS.*	*MEMORANDA.*	*RESULTS.*
	1	For bony anchylosis.	The anchylosis was the result of an old fracture; the arm was extended and useless.	Primary union throughout whole wound under two dressings; functional result, excellent.
13		Amputations.		
	8	Finger.	Six, from lacerated wounds in crushing accident; two, from result of felon.	All healed by first intention under one dressing.
	1	Forearm.	From tubercular arthritis of wrist and carpus.	Primary union under two dressings.
	1	Arm.	In old ununited fracture at the upper third.	Primary union.
	2	Foot.	One malignant tumor of foot, one for crush of foot, both choparts.	One, healed by first intention. Second, recent.
	3	Leg.	All for railroad injury.	Primary union in all.
	2	Thigh.	One for malignant growth of lower end of femur; one for tubercular osteitis of knee.	In one, antiseptic suppuration in track of drainage tube, which was left in situ for an unnecessarily long time. Final healing. In other, primary union under two dressings.
14		Radical cure of hernia.	All of these cases were strangulated, all were seen and operated on early. In the inguinal cases Barker's method was used; in the femoral, a modification of that method.	All recovered from the operation. In two, there were fever and suppuration along the track of the catgut drainage. In one there is already a return of the hernia. In the others the cure *seems* permanent.
	3	Inguinal.		
	2	Femoral.		
15	2	Parotid tumor.	Both cases were examples of tumors over the parotid, originating in the lymphatic rather than true tumors of the parotid itself.	Primary union in one. In the other, a little aseptic oozing for a week.
16	1	Osteotomy and refracture of bones of the forearm.	For angular deformity following badly treated fracture. Open wound.	Primary union under two dressings. Cure.
17	1	Myotomy of adductor, sartorius and tensor vaginæ femoris.	For contractures of long standing.	Primary union.
18		Removal of tumors.		
	11	From scalp.	In eight cases the operation was for the removal of sebaceous growths. In three, included here for convenience, a painful scar was excised for supposed traumatic epilepsy.	In every case there was primary union, except (in three), along the track of the drainage tubes. In no case was there fever or suppuration.
	3	From neck (exclusive of tubercular glands).	Hydrocele of the neck; two malignant growths of neck.	
	4	From back.	Three fatty tumors; one large cyst.	
	1	From thigh.	One large fatty tumor.	
	2	From tongue.	One cyst; one malignant growth.	
	2	From tonsils.	For hypertrophy.	There were no deaths.
19	1	Ligation of brachial at two points.	For ruptured traumatic aneurism; old operation.	Some sloughing from pressure previous to operation. Slow healing afterward, with aseptic suppuration. Case ran a febrile course to entire recovery.
20		Sequestrotomy.	In these cases the cyanide gauze was used freely as packing, and was found to work excellently well. As the bone cavities slowly granulated, the discharges remained sweet, and were serous rather than purulent.	Recovery in each case.
	2	Of femur.		
	2	Of tibia.		
	1	Of cranium.		
21		Ununited fracture.	Both of these cases were operated on by wiring. In the former the implantation of sterilized bone was tried, but was not successful.	One failure; one recovery. No suppuration or fever in either case.
	1	Of humerus.		
	1	Of tibia.		
22		Abscess.	In all of these, antiseptic irrigation with bichloride was used, and recently with peroxide of hydrogen, always followed by packing with either cyanide or iodoform gauze.	All recovered. Profuse suppuration ceased almost immediately, and the cavities rapidly closed during a moderate serous oozing.

TABLE—*Continued.*

Group.	No. of Cases.	OPERATIONS.	MEMORANDA.	RESULTS.
	6	Acute.		
	5	Chronic.		
	2	Of bone.		
23		Perineal section.	In all, the retained catheter was used, and the wound was treated in accordance with the antiseptic methods described in this paper. Cyanide gauze packing was employed when necessary.	Recovery in every case but one, in which nephropyelitis probably occurred. Exploratory nephrotomy was recommended, but refused, and the patient, a child, passed from under observation.
	3	Rupture of urethra.		
	2	False passage.		
	1	Traumatic stricture.		
24		Plastic operation.	Union by first intention in all except two, in which tension caused separation of line of flaps. In those, union by granulation took place.	Recovery.
	1	To make upper lip.		
	2	To make lower lip.		
	1	To remove epithelium of cheek and lower lid.		
25		Anal and rectal operations.	In these cases, either the cyanide or the iodoform gauze was used in packing.	Recovery in every case. A febrile course. No suppuration.
	8	For fissure.		
	6	For hæmorrhoids.		
	10	For fistula.		
	1	For rectal fibroids.		
26		Compound fractures, in which drainage and antiseptic dressings were used.	In these cases sterilized iodoform was used in addition to the gauze.	Recovery in every case. No fever. No suppuration, except in the last case when the injury was crushing, and the damage to tissues excessive.
	3	Skull (included under trephining).		
	1	Humerus.		
	1	Humerus, and both bones and forearm.		
	6	Tibia.		
	4	Both bones of leg.		
	3	Tarsus and metatarsus,		

It will be observed that there are but five deaths in this list, two of them due to pre-existent and chronic disease; the other three were abdominal cases, operated on with the patients almost *in extremis*, with swollen bellies, intestinal paresis, and in one case with suppurative peritonitis. The surgeon who has not lost such as these in spite of his best efforts and most careful application of antiseptic and operative methods has, indeed, been fortunate.

I can truthfully say that in the whole list there have not been six freely suppurating cases, and that there have been no cases of septic trouble, and no deaths from any form of blood-poisoning.

The majority of these cases have been observed from beginning to end by sections of the University class, and have been under the daily care of my residents at the University Hospital. Of most of them I have elaborate clinical reports, made by members of the third year classes, and handed in to my assistants at the completion of the case.

Upon others I have operated in private for well-known physicians. I am sure that all who have seen these cases, whether students or practitioners, will corroborate my statements both as to the harmlessness and the efficacy of the method in question.

I am glad to be able to give this positive testimony in favor of the latest Listerian method, which, while it may not be permanent (as there are yet many opportunities for improvement before the *ideal* dressing is reached), is obviously undeserving the epithets of "deadly and dangerous," applied to it by Mr. Tait, without, I will venture to say, his having taken the trouble to watch its effect in a single instance, to calculate the quantity of cyanide in a single dressing, to consider the improbability of absorption, or indeed to give the matter any serious consideration whatever. For an example of the unscientific spirit which Mr. Tait justly claims for himself, and for absence of the humility which he unjustly pretends to associate with it, as well as for absolute recklessness of statement, commend me to the paragraph I have quoted about the cyanide dressing.

I must not be understood in my eulogies of antisepsis as meaning to depreciate efforts made to attain the surgeon's ideal condition of operative wounds, *i.e.*, perfect asepsis. There is no opposition between them. On the contrary, asepsis is the outcome of antisepsis, but I must confess to grave doubts of the efficacy of many of the means advocated to this end, and some of the testimony in relation to it. These doubts are founded on a comparison between published results and observed cases in the hands of some eminent European surgeons—the almost uniform effect upon my mind of such opportunities of comparison having been a distinct loss of confidence in the statistical reports. I saw, for example, a series of cases of excision of the breast, with pigmented irritable scars, with suppuration, with dressings that were perceptibly stinking, shown to a distinguished surgical audience, the operator and lecturer being a vigorous advocate of asepsis as opposed to antisepsis, and having in his clinic elaborate apparatus for the sterilization of dressings by steam heat under pressure, admittedly the best of all the non-chemical sterilization methods yet discovered. These cases were in such marked contrast to what I had read of the results obtained at this clinic that the experience was a great shock to my faith in current surgical literature, a shock from which it has scarcely yet recovered.

It cannot be disputed in the light of modern science the operator has a two-fold duty, viz., to prevent the entrance of living pathogenic microbes into the wound, and at the same time to preserve the vitality of the tissues themselves. We have already spoken of the phagocytic theory of Metchnikoff[*], and in addition we have learned through Waterhouse's experiments,[*] and through the clinical experience already spoken of, of the anti-bacteric power of the peritoneum. Bouchard and others have observed the destruction of bacilli by the spleen and their elimination by the kidneys, as in typhoid fever. Buchner and Subarsch[†] have shown the bactericidal power of blood-serum or of some substance such as the fibrin ferment contained therein. We have abundant reason, therefore, to believe in a general antagonism between the body cells and the micro-organisms of disease, even if, with our friend, Dr. William Osler,[‡] we are compelled to consider the question of phagocytosis as still an open one. It follows that the theoretical propriety of non-interference with these tissues cannot be doubted, and Lister plainly admitted and accepted this new view when he said that the floating particles of the air having been shown to be less harmful than was supposed, we may possibly dispense with antiseptic washing and irrigation, "provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources."

When this method is adopted it is evident that all strong antiseptic solutions which might compromise the vitality of the tissues must be discarded ; instruments, silk, ligatures, sponges, etc., must be sterilized by heat, hands must be washed clean in sterilized water (after previous chemical disinfection), and the same is true of the skin over the field of operation. If antiseptic solutions are used at all they must be very feeble—1:60 carbolic for instruments (after previous sterilization by heat), 1:10000 sublimate for sponges, etc.

In the meantime, however, while waiting for further improvement in this direction I have felt and still feel that it is safer in a large general clinic, with several assistants and with numbers of students actively participating in the operative work, to depend upon full antisepsis rather than upon asepsis, and I have in the cases above cited and in large numbers of others, not of enough importance to include in the table, employed the following methods, substantially based on those of Lister himself, as described to me in a private letter last January. They represent, therefore, the latest views made public before the Berlin address. I need not detain you with a description of the preparation of the gauze itself. This can be found in detail in *The Lancet* for November 9, 1889, and January 4, of this year.

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[*] Fortschritte der Medizin, Bd. 2, 1884, No. 17.

[†] Virchow's Arch., Bd. 119, heft 3, p. 342.

[‡] Versammlung Deutsch Naturforsch und Aerzte ; Heidelburg [1889.

[§] *Medical News*, April 13 and 20, 1889.

Immediately over the wound is placed a portion of the cyanide gauze, washed in a solution of carbolic acid to get rid of the bichloride of mercury in the dressing in contact with the wound. For this purpose one part of carbolic acid to twenty of water is safer than any weaker solution. A very pure acid should be used, because, if impure, it is not completely dissolved, and the undissolved particles are needlessly irritating to the surgeon's hands. When a dressing is changed this piece of gauze is applied over the wound before the surrounding parts are cleaned with the carbolic solution. The carbolic acid soon flies off from the washed gauze, leaving nothing in it in contact with the wound but the unirritating cyanide.

By proceeding in this way it is not necessary to use "protective." The amount of unwashed gauze to be applied will depend upon the amount of discharge anticipated.

In the early stage of a wound, where we expect much sero-sanguineous oozing, it is desirable to use a considerable mass, say an inch in thickness, and extending on all sides beyond the wound. Further, it is well, where free discharge may occur, to place over the dressing a piece of thin mackintosh with the caoutchouc side (antiseptically washed) next the dressing. This is for the purpose of preventing the discharge from coming directly through the dressing. It should not, of course, overlap the gauze, nor need it even extend to its edge. When the part operated on is placed on a splint, as after resection of the knee, the padding of the splint is lined with such a piece of mackintosh.

It is unnecessary to purify bandages or elastic bands that are placed outside of dressings, but if a bandage is used in the interior of a dressing, as in bandaging a stump next the skin, it is purified sufficiently by soaking it thoroughly with a solution of corrosive sublimate 1:2000. For purifying the skin 1:20 carbolic acid in 1:500 sublimate solution is used. The towels placed around the seat of operation are wrung out of 1:2000 sublimate lotion, and this is also used for the sponges during the operations. The carbolic solution is used for purifying the instruments before an operation. At the end of an operation, before beginning to stitch, the wound may be washed with 1:500 sublimate lotion, and irrigated with 1:4000 during the stitching.

But where a joint, such as the knee, is opened, the use of the strong sublimate solution, which is seriously irritating to the synovial membrane, should be avoided; and in that case it is better to irrigate through the whole operation with 1:4000. As to rendering wounds aseptic which have been infected, that is, speaking generally, a thing on which we can never reckon with absolute certainty; in recent wounds, like compound fractures, the chance of success is, of course, always greater the shorter the time that has elapsed after the infliction of the injury.

If the skin and wound are greasy, oil of turpentine is of great value for cleaning away the dirt, and the use of a nailbrush with carbolic and sublimate solutions, after the turpentine (or without it if there be no special occasion for its use), is very valuable. In many cases where septic sinuses are present, it is hopeless to try to extirpate the septic mischief; but very great advantage is gained by washing the cut surfaces in an operation under such circumstances with a solution of chloride of zinc, forty grains to the ounce of water. This is done once for all at the conclusion of the operation, and prevents putrefaction during the critical early day. The same solution is used where the wound communicates with a septic cavity, as after the removal of a portion of a tongue, or of a jaw; and under these circumstances it is also useful to apply iodoform to the cut surface after the chloride. In cases in which the septically infected part is of very limited extent, as, for example, where a suppurating strumous gland has discharged without an antiseptic dressing, complete disinfection may often be obtained by applying undiluted liquid carbolic acid, after scraping away the degenerated glandular or other tissue with a sharp spoon.

I have recently learned to value peroxide of hydrogen very highly as an antiseptic, especially in suppurating cases and am using it more largely than ever before.

This address has already assumed an inordinate length on account of the necessity for free quotation, but I cannot conclude it without a protest against the tone of Mr. Tait's paper and against the personal virulence of its references to Lister. There is, unquestionably, room for legitimate differences of opinion as to the treatment of operative wounds; there are many problems relating to the principles involved which are yet unsolved; the precise comparative value of the different factors, which taken together, have given modern surgery its scientific exactness, is yet to be determined; argument and discussion are not only excusable but highly desirable and, conducted in a proper spirit, could scarcely fail to be productive of great benefit to surgery and humanity. But this is not to be brought about by invective or by personal detraction. Mr. Tait discloses his real animus when he complains that for twelve years he has been "ignored" by Lister, and his controversial writings show that he probably belongs to that class of persons who feel far more deeply injured by neglect than by assault.

It is doubtless true that he and his work, carried on in a special line and under special conditions, have not in the consideration of the whole subject been elevated into the prominence which he thinks they deserve; but how many of us in this world are taken at our own valuation, either personally or professionally?

Lister's work, since he took his first degree in 1852, has been of a character to command the respect and admiration of the scientific world. Receiving the first prizes in botany and anatomy when he passed the M.B. examination, he won the gold medal and scholarship in surgery in the final examination. His papers on the minute anatomy of the skin, the physiology of the lacteal current, the contractility of the iris, the microscopic anatomy of involuntary muscular fibre, the relations of the inhibitory system to the visceral nerves, the regulation of arterial contraction by the nervous system, the early stages of inflammation, the coagulation of the blood, etc., etc., stamped him at once as an original investigator of the first rank, and as one of his biographers says, "would have sufficed to make his career memorable if he had never applied antiseptic measures to the treatment of disease." I have heard Prof. Louis Agassiz years ago, and later Prof. Joseph Leidy, say that, viewed from the standpoint of abstract science, his work had been of the very highest order, and the appreciation of his labors by the best minds in our own profession has been enthusiastic and almost universal. Nearly every great surgeon in the civilized world has put on record his admiration for Lister's teachings, his acceptance of the general principles involved, and his sense of almost personal obligation to the author of the antiseptic theory.

That Mr. Tait should speak ot such a man as having "lived in the clouds of his spray for the last twelve years," as "wanting in logic," having "crude notions of logical definition," making "illogical blunders," "falling away from his own faith," promulgating an "absolute and ludicrous logical error," etc., and should make a boast of having "laughed at" and "ridiculed" him and his doctrines and disciples, is, it seems to me, evidence of his unfitness by temperament or training (or from lack of the latter) for the serious discussion of broad surgical principles. I am quite sure that the vast majority of general surgeons will be found to have no sympathy with either his views or his manner of expressing them, and it is a relief to find that in his own special line there are operators of equal eminence who repudiate both. In an address on "Abdominal Surgery, Past and Present," recently delivered before the Medical Society of London, its president, Mr. J. Knowsley Thornton said: "I am not ashamed still to use the spray and all the precautions which have advanced my results in ovariotomy to 1.88% mortality,* and I find increased practice and a steady adherence to methods which have yielded me good results in the past, increase in like ratio my success in all abdominal operations. Every operator of prominence improved his results enormously as soon as he adopted Listerism; then having learnt how to be surgically clean, he has found for himself ways of attaining this end with more or less success by methods differing from those of Lister. The sum and substance of it all is, that if we had never had Lister to teach us true cleanliness, we should never have used antiseptics, flushings, or drainage tubes to attain it. The great advance is due to the antiseptic system, the minor details are merely the different ways of attaining the same end—asepticity. Time alone will show what is worth retaining and what we may safely cast aside."

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*As against Bantock's 4 and Tait's 3.3

THE GERMS OF TYPHOID FEVER.

By VICTOR C. VAUGHAN, M.D., Ph.D.

A Lecture delivered at the Post-Graduate Course of the University of Toronto, Dec. 19, 1890.

IN 1880, Eberth,[1] of Zurich, in eighteen out of forty cases of typhoid fever, found in sections of the spleen and mesenteric glands a short bacillus which he was unable to detect in similar examinations of twenty-four persons who had died of other diseases. From these facts this distinguished Swiss investigator thought himself entitled to the claim of the discovery of the true germ of typhoid fever.

Before Eberth's report, Koch had observed this germ, and had micro-photographs of it. Indeed before Koch,[2] Browicz,[3] Sokoloff,[4] and Fischel,[5] had individually found in the tissues of typhoid subjects oval bacilli which were probably identical with those of Eberth and Koch. This organism is designated by some as the Koch-Eberth germ, and by others as the Eberth germ, or bacillus. For the sake of brevity, I shall in the following discussion employ the shorter name.

This organism is now regarded by the majority of bacteriologists as the true and only cause of typhoid fever, and I shall give my attention first to the grounds upon which this belief is founded. Has it been conclusively demonstrated that the Eberth germ is the sole and sufficient cause of typhoid fever? To what extent have the four rules of Koch been complied with in the study of this germ? Are there any reasons outside of these rules for founding a belief either for or against the specific nature of this bacillus?

In the first place, is this organism found invariably in typhoid fever? As has been stated, in his first report Eberth found it in eighteen out of forty cases; but since that time methods have been perfected and many other investigators have studied this point. In the examination of twenty-eight bodies, dead from typhoid fever, Gaffky[6] found the Eberth germ in twenty-six; Frankel and Simmonds[7] reported it in twenty-five out of twenty-nine cases; Seitz in twenty-two out of twenty-four; Rietsch[8] in thirty-five out of thirty-six; and Kowalski[9] found it present in each of twenty-nine cases.

Without citing further authorities, I may say that the number of cases of genuine typhoid fever in which this germ has not been reported, when the examinations have been made by competent men, is so small that we are justified in claiming that the first of Koch's rules has been complied with. Indeed, in reading over the great number of cases referred to in the volumes of Baumgarten's "Jahresbericht," in which the Eberth germ has been reported to be present, one becomes somewhat suspicious of the universality and readiness with which this organism has been detected. Gaffky and others found it always present in the mesenteric glands and spleen, often in the liver, and not so frequently in the kidney. In the intestine it has been found, in the early stages, in the swollen follicles and plaques; and in the deeper layers, before there is any necrosis; but with the advent of ulceration there is found secondary invasion. But in addition to these organs, the Eberth germ has been reported by Chantemesse and Vidal[10] in the lungs of typhoid patients with bronchitis, broncho-pneumonia and pneumonia. The same observers report the germ in the brain; Curschmann,[11] in the spinal cord; Zenker and Hoffmann,[12] in the voluntary muscles and in the marrow of the bones; and Reber,[13] Neuhauss[14] and Chantemesse and Vidal, in the placenta of typhoid patients. In twenty samples of blood taken from the finger, during life, of typhoid patients, Meisels[15] reports the finding of this germ in nineteen. Neuhauss examined the blood taken from the eruptive spots in typhoid fever with success. Maragliano[16] and others examined the blood taken from the spleen during life as a means of diagnosis. The Eberth germ has been found in drinking-water by Dreyfuss-Brisac and Vidal,[17] Michael,[18] Moers,[1

Chantemesse, Loir,[20], Thoinot, De Blasi,[21] Galbucci,[22] Beumer,[23] and others. Granting that these observers have not been mistaken in the nature of the germs which they have found, we must admit that the Eberth germ is widely distributed. Banti[24] reports Eberth's germ in a case which he calls atypical typhoid fever, and in which the intestine was perfectly sound. This case is pronounced typhoid fever, notwithstanding the absence of any indication of the characteristic lesion of the disease, because the germ was found to be present. Whether or not this is justifiable is, I think, very questionable.

That the second of Koch's rules has been complied with, there is no doubt. Pure cultures of this germ are to be found in every bacteriological laboratory. It grows readily in our artificial media, and its (accredited) characteristics of growth are well known. The attempts which have been made to induce the disease in the lower animals by inoculation with this germ are numerous and interesting. I shall not attempt to review all of these, but will call your attention to some of the more important ones. The first to experiment with pure cultures was Gaffky. To five Java apes, he fed daily cultures without any results. Later these animals died of tuberculosis, and section showed no lesions resembling those of typhoid fever. Equally without result were two experiments upon apes; in one, the inoculation being made directly into the blood, and in the other, in the tissues over the sternum. Gaffky also inoculated rabbits, guinea-pigs, rats, mice, pigeons, a chicken, and a calf, all without result. In these, the inoculations were made directly into the blood vessels or into the peritoneal cavity.

The experiments of E. Frankel and Simmonds[25] seemed at first to prove that the Eberth bacillus is pathogenic to some of the lower animals. These experimenters suspended the germ taken from potato cultures in sterilized water and injected this either directly into the blood or into the peritoneal cavity. Nineteen, out of thirty-two rabbits, and twenty-seven, out of thirty-one mice, succumbed under this treatment. These animals survived the injections from a few hours to four days, the majority dying on the first day. The symptoms observed consisted of lessened response to surroundings, lessened inclination to move, loss of appetite, and, in many, diarrhœal discharges. Section showed enlargement of the spleen and mesenteric glands, and swelling of the intestinal follicles. In some there were observed enlargement of the axillary and inguinal glands, ecchymoses of serous membranes, inflammatory redness and hemorrhage in parts of the intestines, and parenchymatous swelling of the liver and kidneys. In no case was there suppuration at the point of inoculation. In the spleen, mesenteric glands, liver, kidneys, and Peyer's patches, the bacillus could be detected. However, the abundance of the bacilli did not correspond with the extent of the macroscopic changes. From these experiments, Frankel and Simmonds concluded that they had successfully transmitted typhoid fever to these animals.

Independently of the above, Di Vesta inoculated, with fatal results, rabbits and guinea-pigs. The lesions were not so marked as those found by the Hamburg investigators. Later, A. Frankel[26] inoculated guinea-pigs with the Eberth germ after the manner employed by Nicati and Rietsch in their experiments with the cholera bacillus, and which consists in introducing the germ directly into the duodenum, either with or without ligature of the bile duct. Of the fourteen animals thus treated, seven died. The anatomical changes observed were similar to those found by E. Frankel and Simmonds.

Seitz,[27] employing Koch's method for producing cholera in guinea-pigs, found that seven out of sixteen animals died within forty-eight hours, and one other after four days. The germ could be detected in the intestines, but not in the other organs. Intravenous and intraperitoneal injections of the germ produced results similar to those obtained by E. Frankel and Simmonds.

Beumer and Peiper[28] confirm the observations made by E. Frankel and Simmonds, and A. Frankel and Seitz, but interpret their results in a wholly different way, and reach a diametrically opposite conclusion. In the first place, they find that the effect upon the animal is in direct proportion to the amount of the culture injected; an amount which is small, but which still contains many thousand germs, fails to produce any symptoms. Secondly, they prove that the Eberth germ does not multiply in the body of the animal, but, on the contrary, soon dies. Thirdly, they show that the same symptoms and identical structural changes result from injecting like amounts of certain well-known non-pathogenic bacteria, such as bacillus subtillis, the greenish-yellow liquefying bacillus of water, the white bacillus of water, and others. Thus they show that the death of the animals is due to intoxication, and not to infection ; also, that the intoxication is not specific, inasmuch as it is produced with equal facility with other bacteria.

Finally, Sirotinin[29] demonstrates that the same symptoms and pathological lesions are produced by sterilized and non-sterilized cultures of the Eberth germ. This completes the proof that the effects observed are not due to the growth of the germ within the body.

To sum up the evidence which we have on this point, we may say : (1) A germ giving the tests supposed to be characteristic of the Eberth germ is found invariably in the bodies of those dead from typhoid fever. (2) It has been isolated and grown in pure cultures. (3) All attempts to induce typhoid fever in the lower animals by inoculation with this germ have so far been without success. (4) Experiments show not only that the germ fails to multiply in the lower animals, but that, when introduced by inoculation, it soon dies.

I think that every bacteriologist will agree with me in the conclusions which I have just drawn ; but some of them will say with Baumgarten[30] that the lower animals do not have typhoid fever, while others agree with Welch[31] that it is not necessary to reproduce the disease in animals in order to prove the causal relation between the germ and the disease. Welch states : " From our present knowledge, we are justified in believing that a micro-organism which is invariably associated with a disease, which is found in the lesions of the disease and in situations which explain the symptoms and lesions, and which is never found except in association with the disease, must be regarded as the cause of the disease." By one or the other of these assertions the majority of bacteriologists satisfy their scientific minds and accept Eberth's germ as the true, sufficient, and sole cause of typhoid fever. Neither of these explanations has been satisfactory to me, and we will briefly consider their weight.

In the first place, do any of the lower animals ever manifest the symptoms and demonstrate the lesions of typhoid fever? In discussing this point, we will leave out of consideration for the time being all questions about the identity of the germ which may cause typhoid fever in man and that which may cause the same disease in the lower animals. In fact we will not, for a few moments, say anything concerning germs, but will endeavor to answer the plain question, are the lower animals wholly free from typhoid fever? In the first place, it will be necessary for us to know just what symptoms and lesions are necessary in order to constitute typhoid fever. The only constant symptom is the continued fever. There may be either diarrhœa or constipation ; the eruption either may or may not appear; active delirium is frequent in some epidemics, while in others the mind is perfectly clear throughout the course of the disease. In short, as we all know, there are the greatest variations in the symptoms among which the only constant and invariable one is the continued fever. The only constant lesions are the inflammations of the solitary and agminated glands of the ileum, often progressing to ulceration, and the enlargement of the spleen and mesenteric glands. If a man should manifest a constant elevation of temperature extending over from seven to forty-two days, then should die, and a careful *post mortem* examination should reveal ulceration, possibly perforation, of the ileum, and enlargement of the spleen and mesenteric glands, with no tuberculosis, what would any intelligent jury of medical men give as the cause of death? Could it be other than typhoid fever ? Now, if this symptom and these lesions are observed in a dog or cat, what shall we name the disease? These symptoms and these lesions may be induced and do occur in the lower animals.

Huguenin, as quoted by Murchison, says that enteric fever with characteristic intestinal lesions is by no means uncommon among cattle in Switzerland.

In 1882, Brautlecht[32] inoculated rabbits with germs from water supposed to be the cause of a typhoid epidemic, with the following results: The animals had some elevation of temperature, lost flesh until they became mere skeletons, and died after from two to eight weeks. Very young animals died within two or three days after having profuse diarrhœal discharges. *Post mortem* examination showed in the adult animals invariably a severe catarrh of the small intestines, and marked enlargement and coloration of the spleen and mesenteric glands. The walls of the intestines were thickened and friable, the mucous membrane, swollen, yellowish, and reticulated. Ulceration was present in some cases, but was generally confined to individual glands.

In 1885, Semmer[33] reported typhoid fever in three dogs and a horse. The lesions were found to be well marked. In 1888 he confirmed his statement regarding the occurrence of typhoid fever among the lower animals, and reported finding in some of these a germ similar in form and size to that of Eberth, and in others an organism which differed from this only in being larger.

Drs. Hendricks and Brewer made the following report on the *post mortem* examination of a dog which died on the twenty-eighth day after I had inoculated it with a germ obtained from water supposed to have caused typhoid fever: "The weight of the body was twenty pounds. Upon the abdominal cavity being opened, the mesenteric fat was observed to be pink, with hemorrhagic spots. The blood vessels of the intestines were found to be highly injected, with numerous points of hemorrhagic infiltration. On the mesentery near the cæcum were large hemorrhagic spots. The lower inch and a half of the ileum was distended and covered externally with clotted blood. The mesenteric glands were enlarged to four or five times their normal size, and each contained numerous hemorrhagic spots. The glands of the vermiform appendix were much enlarged, and were pitted in the centre. Under the mesentery of the cæcum was a plastic exudate, almost transparent. The descending portion of the duodenum showed numerous bright hemorrhagic spots along the line of its junction with the mesentery. Along the upper portion of the ileum the intestine presented a greenish macerated appearance, and a soft, pultaceous feel. Air blown into the intestine escaped through the walls at this point. The mucous membrane of the ileum was swollen and Peyer's patches enlarged, but there were no distinct ulcerations. The liver was indurated and of a deep cherry color; weight, twenty-three ounces. The kidneys were enlarged to twice the normal size, and were dark in color. The spleen was enlarged, and of a dark purple color. The stomach was contracted, and its mucous membrane showed a few hemorrhagic spots. The lungs were normal, and the heart in diastole and free from clots. The thymus and lymphatic glands were enlarged."

Flügge[34] says: "It is known that a group of widely distributed organisms, which, however, are wholly different from the typhoid bacillus, have the power, when injected subcutaneously or intravenously, of producing in animals death, with marked swelling and ulceration of Peyer's patches. To these organisms undoubtedly are due the apparently positive results which some authors have supposed to be due to inoculation with the typhoid bacillus."

It is unnecessary to bring forward further evidence on this point. It must be acknowledged that members of several species of our domestic animals occasionally manifest the essential symptoms and lesions of the disease which in man we designate as typhoid fever. This disposes of one of the two explanations given by some bacteriologists for the failure to induce typhoid fever in the lower animals by inoculation with the Eberth germ, and we will proceed to a discussion of the second explanation.

When we say that Eberth's germ is invariably found in the spleen and mesenteric glands of those dead from typhoid fever, we understand that this germ possesses certain characteristics either in form, size, reaction to staining agents, in manner of development or growth, or in some other particular by which it can be distinguished from all other germs. If it can be shown that the organism has no such characteristics, or that its supposed characteristics are possessed and reproduced by other germs, then all the evidence of its specific nature which has been drawn from its constancy in typhoid cases becomes worthless, because no investigator can then say that the organism which he finds belongs to the same species which Eberth observed; for the means of recognizing this species to the exclusion of all others do not exist, or at least have not been applied.

What are the tests upon which we rely in the identification of Eberth's germ? No bacteriologist will, I suppose, claim that there is anything in the form or size of Eberth's germ by which it can be distinguished from the ordinary saprophytic bacilli; but on this point I will fortify myself with a quotation from Baumgarten, a most earnest believer in, and a most able exponent of, the specific character of the organism. He says : "The typhoid bacillus in those development forms in which we most frequently see it (namely, in the infected body of man) shows a very great resemblance to the common, widely-distributed bacilli of ordinary putrefaction which were formerly designated by the now antiquated name of bacterium termo. In the fresh typhoid infiltrations of the intestinal walls, in the typhoid, infiltrated mesenteric glands, in the hyperplastic spleen, in the liver, in the kidney, etc., our bacilli (Eberth's germ) appear just like (gleichwie) the bacterium termo, as short (scarcely three times as long as broad), relatively thick rods with rounded ends, which are often joined in twos, at times in threes, so as to appear on a superficial examination like long single bacilli." We therefore see that it is admitted by those who believe in the specific nature of Eberth's germ that there is nothing in its morphology by which it can be distinguished from certain putrefactive bacilli. We turn, therefore, from this point and now proceed to the study of certain tinctorial and growth properties, by means of which Baumgarten and others state that Eberth's germ can with absolute certainty be distinguished from the bacilli already mentioned. Great stress has been laid upon the statement that Eberth's germ takes the analine colors with difficulty and imperfectly, while the putrefactive bacilli take such stains promptly. I once carried to one of the most renowned teachers of bacteriology in Germany cultures of a germ which I had isolated from drinking-water, and after inoculation with which three dogs had manifested continued fever, one of the animals dying on the twenty-eighth day, a second on the thirty-fifth day, both showing on *post mortem* examination in a marked degree the essential lesions of typhoid fever, and the third finally recovering, and asked him if it might not be a modified form of Eberth's germ. He found that my germ did take the ordinary stains, and from this fact alone claimed that it could have no relation to Eberth's germ, that it was nothing more nor less than a saprophytic bacillus, and that either it had no causal relation to the diseased condition in the dogs or the disease in the dogs was not typhoid fever, "Because," said he, "there is no doubt that Eberth's germ is the specific cause of typhoid fever." I think that I am now in a position to demonstrate that this test is wholly unreliable. In the first place, the behavior of Eberth's germ to staining reagents is wholly dependent upon circumstances, and will vary greatly with different samples. This is partially recognized by Baumgarten, who states that the difference in the readiness of taking stains between Eberth's germ and the putrefactive bacilli is less marked in cover-glass preparations than in sections. Eberth's germ, which has been grown artificially through many successive generations, takes the stain quickly and deeply. I have here preparations made from a pure culture obtained in the Hygienic Institute of Berlin in August, 1888, and that these bacilli have taken the stains to which they were exposed for less than a minute can be readily seen. In the tissues, or in cultures recently taken from the tissues, this germ is stained with difficulty ; but when long grown outside the body, it takes these same stains readily. Now, what condition leads to this difference ? I have here two germs taken from drinking-water, which as first obtained are stained quickly and deeply, but after they have been kept at a fever temperature for a time, they manifest the same difficulty in staining as is shown by Eberth's germ. This difficulty in staining increases directly with the temperature and time of exposure. Moreover, readiness in being stained is not immediately regained by succeeding generations of the heated germ, even when these successive generations are grown at ordinary temperature. I will present to you preparations of these germs along with those of Eberth submitted to the action of the same stains for the same length of time, and I am sure that no one will be able to distinguish one from the other by any difference in depth of stain.

Great stress has been laid upon the fact that Eberth's germ is not stained by Gram's method, while the putrefactive bacilli are stained by this method. This apparent difference is also due to differences in temperature at which the germ has been grown. I have here preparations of a bacillus obtained from drinking-water, which, after having been kept at from 39° to 40°C. for from three to twenty-one days, are wholly unaffected by Gram's method of staining. I must therefore conclude that the method of distinguishing Eberth's germ from certain putrefactive bacilli by differences in behavior towards staining reagents is wholly unreliable.

I will submit to you numerous preparations of two germs obtained from water along with similar preparations of Eberth's germ, the length of exposure to heat and the degree of heat being indicated on each slide, and I am sure these preparations will bear me out in the above conclusion.

We have been taught that the invisible mould which Eberth's germ forms on potato is an absolutely certain means of its identification and distinction from all other germs. Frankel says that this "is a sure means of correct recognition, for the growth is perfectly characteristic and does not occur with any other known germ." This positive statement is reinforced in the same paragraph as follows: "This growth of the typhoid bacillus is so characteristic that with its help one can at any time distinguish this from other bacteria, and one should not report the presence of this germ until every doubt has been removed by the potato culture."

Baumgarten also states that the growth on potato is wholly characteristic, and gives this method of distinguishing Eberth's bacillus from that of Emmerich (a fæces bacillus), which, as he states, agrees with the Eberth germ in its growths on gelatine, agar, and blood-serum. Now, the two germs which I have here, and which I obtained from drinking-water supposed to have caused typhoid fever, grow on potato, when they have not been kept at a fever temperature, much like the fæces bacillus, forming a raised whitish mould; but after having been kept for seven days or longer at from 39° to 40°, they form a wholly invisible growth on the potato which cannot be distinguished from Eberth's germ. Proofs of this statement are here before us, and I am sure that no one could select out the potato which bears Eberth's germ.

It will be well to speak somewhat in detail concerning these germs which I have obtained from water, and also of the exact conditions to which they have been subjected in order to so modify them that they are affected by staining agents in the same manner as Eberth's germ, and also produce a like growth on the potato.

In October, 1888, I adopted the following method of making a sanitary analysis of drinking-water: In all cases where the samples are collected by the laboratory assistants, a sterilized flask closed with cotton is used. In other instances, when I am informed by a health officer that he desires the analysis of a water, I send him a sterilized bottle, or instruct him how to sterilize a bottle. In other cases, the water comes usually in a new bottle or jug which has been well rinsed with the same water. As soon as the water is received, plate cultures are made and a test tube of beef-tea is inoculated with one drop of the water. This tube is placed in an incubator and kept at 37° C. for twenty-four hours. Then twenty drops of the beef-tea cultures are injected with a sterilized syringe into the abdominal cavity of a white rat or guinea-pig. In some instances rabbits have been used, and with these the amount injected has varied from twenty to sixty drops, according to the size of the animal. If the water contains a toxicogenic germ the animal dies, usually within twelve hours. A *post mortem* is made, the gross appearances noted, and plates are prepared from the spleen, liver, kidney, and sometimes from the blood. After twenty-four hours longer these plates have generally developed (in some instances a longer time is required), and they are compared with the plates made directly from the water. On the plates made directly from the water the germs are counted and their general appearance noted after twenty-four, forty-eight, and seventy-two hours. In the meantime the chemical analysis is completed, and under favorable circumstances the report can be made three days after the water has been received; a week furnishes all the time required in any case. The germs taken from the plates can then be studied at leisure.

In this manner 160 samples of drinking water have been examined. Of this number, twenty-four were supposed to be causing at the time typhoid fever. In all of these, with the exception of three, toxicogenic germs have been found. I will now give a short description of each of these micro-organisms:

Bacillus A.: This is a short, motile bacillus, which does not liquefy gelatin, and which forms a white, raised, moist growth on potatoes. It forms no gas, takes the ordinary stains fairly, and gives a positive result with Gram's method. Of six full-grown rabbits, each inoculated by the injection of sixty drops of a twenty-four hour culture in beef-tea into the abdominal cavity, all died. Two rabbits which were given subcutaneously twenty drops each of this culture were not affected. Of fifty rats which were given twenty drops of this culture by injection into the abdominal cavity, all died. Two rats which were inoculated subcutaneously died after ten days. Of eight guinea-pigs inoculated in the abdominal cavity, all died; and a like result followed the subcutaneous inoculation of the animals.

Cultures of this germ were tested along with similar cultures of bacterium coli commune, and the former appeared to be slightly more pathogenic with rats and guinea-pigs, and less so with rabbits than the latter.

In every case of death after inoculation with this germ, plate cultures were made from the spleen, liver, and kidney, and in many instances from the blood of the heart, and without exception the plates showed abundant growths; therefore, this germ not only produces death, but is capable of living in the rat for ten days.

The *post mortem* appearances vary with the time which elapses between the inoculation and death. In cases dying within twenty-four hours the intestines are, as a rule, slightly reddened; the liver, spleen, and kidneys engorged; and the heart has invariably been found in diastole. In cases which survive for several days there may be ulceration of the intestine; the spleen and kidney are enlarged, and the liver dark. In a few instances, all of which, strangely enough, have followed subcutaneous inoculation on the back, a marked peritonitis has been found after death. The same fact has been observed with bacillus B. In no instance has suppuration resulted from an inoculation.

In the majority of samples of drinking-water in which I have found bacillus A, no other organism has developed on the plates. In other words, the water is a pure culture of this germ. It may be of interest to say something about the cases of typhoid fever which were supposed to be due to the waters containing this germ.

Of the cases of typhoid fever supposed to be due to one of these waters I had personal knowledge. First, a girl of sixteen was attacked, and I was called in attendance. I inquired concerning the drinking water, and was told that the hydrant water only was used. I could not believe that this water was the cause of the fever, but ordered that all water used in the house should be boiled. I was asked if the disease was contagious, and replied in the negative; but having in consideration the comfort of my patient, advised that two young brothers and a girl visiting at the house should be kept away from the sick room. Within eight or ten days one of the brothers came down with the fever, and we learned that the visitor, who had gone to Cincinnati, was suffering from the same disease. Of course, the mother now claimed that the disease was contagious. Believing that there must be some local cause, I examined the house throughout, but without satisfaction. A little later, a young man rooming in the house was attacked. By this time the first patient, who had been continuously delirious for three weeks, had become rational. When asked about having used any other than the hydrant water, she stated that she always used the cistern water to clean her teeth, and had also used it in a nasal douche, and in spraying her throat. The brother and the student also remembered that they had occasionally drank the cistern water.

The examination of the water was immediately undertaken, and it was found to be exceedingly bad chemically, and in number and kind of germs. The stools of two of the patients were then examined, and the same germ found so abundantly in the water was recognized in the stools. Of course, I cannot say that this germ was the cause of the disease. Fortunately, all of these cases finally recovered, and there was no opportunity to examine their spleens. I cannot refrain from mentioning a few facts concerning the symptoms. Delirium was a marked and persistent feature; it was not the low muttering delirium, but wild, causing the patients to attempt to get out of bed. The fever ran forty-two days. There was no eruption over the abdomen.

A sample of water from Wyandotte, Mich., illustrates how a source of infection may continue to spread the disease through months. Dr. E. P. Christian reports eleven cases occurring among those who used the milk furnished by one man. The first case occurred July 13th and the last Nov. 15th. Dr. Christian sent me some of the water from the well of this milkman, also some of the milk. The water was a pure culture of bacillus A, which with three other germs appeared on the plates made from the milk; while from the organs of the animals inoculated with the milk, bacillus A in pure culture was obtained.

Bacillus A, as obtained from water, differs from Eberth's germ in the following particulars:

(1) It is somewhat shorter and thicker than the Eberth germ which I obtained in the hygienic institute of Berlin. In the majority of the samples the length is about twice the breadth.

(2) The readiness with which the ordinary stains (as Bismarck brown) are taken is very variable; the germs from some of the waters taking the stains readily, while others are not so deeply stained as the Eberth germ.

(3) It is stained by Gram's method.

(4) It forms a plainly visible, white, raised, moist growth on potatoes.

(5) The colonies on the gelatine plates are very uniform, quite generally spherical, and never spreading over the surface.

(6) It grows on gelatine plates more rapidly than Eberth's germ.

(7) Its toxicogenic properties are greater than those of the Eberth germ.

I have studied the effect of heat on this germ in the following manner: A man, while drunk, fell down stairs and fractured his skull, from which injury he died in about three hours. The spleen was removed with aseptic precautions. With sterilized knives it was cut into pieces, which were placed in sterilized Esmarch dishes. Three pieces were inoculated with bacillus A from as many different sources, two with bacillus B, and one piece was left without inoculation. The dishes with their contents were placed in an incubator and kept at from 39° to 40°. Plate cultures were made from these pieces from time to time, and the germs thus obtained were compared with the bacilli as obtained from the water, and with Eberth's germ. The plates from the uninoculated piece of spleen have invariably remained sterile, showing that the organ contained no germs. As these cultures were made from time to time, the above mentioned peculiarities of growth, by which bacillus A had been so easily distinguished from Eberth's germ, were found to become less and less prominent, and finally they have so far disappeared that they can no longer serve for purposes of distinction between the organisms. After only seven days bacillus A has been so altered that its growth on potato can no longer be distinguished from that of the Eberth germ, with which the comparison was made. Bacillus A becomes less and less receptive to the ordinary stains, and I will show you slides made from the spleen after fourteen and sixteen days which have not taken any of the aniline stains, not even the carbolic fuchsin, as deeply as the Eberth germ. This is true even after the altered bacillus A has been exposed to the stain for seventy-two hours, while the Eberth germ was exposed less than five minutes to the same stain. Gram's method ceases to produce positive results. The length of the germ increases in proportion to its breadth, and in some cases long filaments have appeared. I have been much impressed in carrying on this work with the longer time necessary for colonies to develop on plates after heating the germ. It is generally stated, and I had always, before making these experiments, believed, that pathogenic germs develop more tardily than the ordinary saprophytic ones; but the longer this germ is kept at a fever temperature, and the higher that temperature is, the more tardily do the colonies appear on the gelatine plates. In short, I have been forced to the conclusion that the so-called characteristics of Eberth's germ are not inherent in the organism, but result from the effects of the environment under which the organism has grown in the body.

Bacillus B.: This is also a short motile rod, which in cultures from four to six weeks old shows some liquefaction of the gelatin. It is certainly a slowly liquefying germ. It sometimes forms gas along the line of the stick. This occurs, if at all, during the first few days of growth, and long before liquefaction begins. It forms a white, raised growth on potato. The ordinary stains are taken readily, and Gram's method yields positive results. This germ agrees very closely with bacillus A in its pathogenic properties and in the *post mortem* appearances induced, so closely, indeed, that it will not be necessary to repeat details. This germ has, in the few instances in which it has been tried, failed to kill rats when inoculated subcutaneously, though it is fatal to guinea-pigs when employed in this manner. The plates prepared from the organs of animals killed with this germ have invariably yielded numerous colonies. The germ is also found in the blood.

As has been stated, bacillus B was subjected to the same line of experimentation as A, with practically the same results, and a detailed repetition is not necessary. It should be remarked that the chief points of difference between A and B, as well as those between these and Eberth's germ, have disappeared in B, as obtained from the pieces of spleen. Stick cultures in gelatine tubes have not been found to generate gas or to liquefy gelatine.

From these experiments I must conclude that the evidence of the existence of a specific and individual germ which is the sole and only cause of typhoid fever is not at present conclusive. On the contrary, I am strong in the belief that there exists a family or class of germs, any one of which may cause the essential symptoms and lesions of that disease or group of diseases which we now designate by the name of typhoid fever. It may be that in the future we shall be able to differentiate from the symptoms the diseased condition caused by one of these organisms from that caused by another; but at present we are unable to do so.

It seems to me that the view expressed above is supported by the clinical history of the disease. As has been already remarked, the symptoms of this disease do not show the constancy and invariability characteristic of a truly specific disease, such as anthrax or small-pox; but on the other hand, the symptoms show marked variations in their appearance and in their gravity.

I may state that I do not stand alone in questioning the specific nature of Eberth's germ. Some months ago Rodet and Roux[35] found that the bacterium coli commune, when cultivated at a temperature of 44° to 46°, undergoes changes which cause it to bear a remarkable resemblance to Eberth's germ, and they conclude from their work that the latter is only a degenerative form of the former. If this be true, the stools of healthy persons, when they contaminate our drinking-water, may cause typhoid fever, and the existence of a preceding case is not necessary in order to cause the disease.

Chantemesse and Vidal[36] state that Eberth's germ will grow on gelatine containing 0.25 per cent. of carbolic acid, while all other water germs fail to grow on this medium. They therefore propose this as a method of isolating the typhoid bacillus. Such gelatine was prepared, and tubes inoculated with A, B, and Eberth's germ were poured on plates. A and B grew more vigorously on these plates than did the Eberth bacillus. The growth of the Eberth germ was markedly retarded, while that of the others was not.

Thonoit[37] modifies the above test by adding the carbolic acid to the water to be examined, and after a time inoculates ordinary gelatine with the water. He claims that after this treatment only the Eberth germ will develop. This test was applied to natural waters containing A and B, and their development was not affected. The water was allowed to stand three hours after the addition of the carbolic acid.

Holz[38] prepares a gelatine from the expressed juice of raw potatoes, and claims that the Eberth germ will grow on this medium while all similar organisms fail to do so. Such gelatine was prepared according to the directions given by Holz, and on this A and B grew beautifully.

I may remark, in conclusion, that it seems very strange to me, after going over the literature of the subject, that so much stress should have been placed, by the compilers of books, on the means of recognition of the so-called Eberth germ by its behavior towards staining agents. This so-called characteristic is not warranted by the statements of the original investigators. It is true that Eberth makes this an important means of recognition, and states that the bacillus is not colored by Bismark-brown and hæmotoxyln and only slightly by the other ordinary stains; but what is the verdict of others on this point? In the paper in which he calls attention to the fact that he had photographed the short bacillus before Eberth had published his discovery, Koch says: "Eberth states that these short bacilli have but slight tendency to take stains. The photograph here given shows to the contrary, that in taking stains these bacilli are but slightly inferior to other bacteria." Fischel stained easily with hæmotoxyln. Coats[39] had no difficulty in staining with Bismark-brown. Gaffky states that the best color is methylen-blue, but that the bacilli are stained very well (sehr gut) with methyl-violet, gentian-violet, Bismark-brown, and fuchsin, and less well by hæmotoxyln. Meyer[40] had the same difficulty in staining reported by Eberth; but Friedlander[41] found that the bacilli in the sections stained intensely (intensiv).

The conclusion which necessarily follows is that either the action of the organism towards stains is variable and dependent upon conditions, or the majority of the above-mentioned investigators were working with germs which were not identical with those of Eberth.

Since the above lecture was given, a very valuable paper on the "Variability and Varieties of the Typhoid Bacillus," by the distinguished bacteriologist, Babes,[42] has appeared. This paper in part confirms and in part complements the work reported in the lecture. The chief points made in the paper may be stated as follows:

(1) The bacillus varies in size and appearance according to the culture medium on which, or the temperature at which, it is grown.

(2) Many of the saprophytic bacilli stain as feebly in Gram's method as the typhoid germ.

(3) The growth on potato is not always invisible, and many other organisms do form an invisible mould; therefore this test is not characteristic.

(4) The flagella discovered by Loffler are not characteristic.

(5) There is nothing characteristic in the effects upon the lower animals. The "typhoid-like" bacilli are more highly pathogenic than the typhoid germ.

(6) He has found in a case of dysentery, in the body of a mouse dead from an unknown cause, and in water, germs which give all the so-called characteristic reactions.

(7) He has found in twelve *post mortems* of typhoid subjects germs, all of which respond to the characteristic tests, but which differ from one another.

(8) In all of these cases, with one exception, he finds what he calls the typical typhoid bacillus along with atypical ones. This "typical" germ is not identical with that obtained from the Berlin Hygienic Institute, and among its properties are two which other bacteriologists would deem sufficient to exclude it from any possible connection with the typhoid germ. These are: (1) it evolves a putrefactive odor*, and (2) old cultures on potato are brown.

(9) Although he has been unable to convert any of the varieties into the typical germ, he has seen the differences grow less marked. (We are not told what means he has used to influence the properties of the germs.)

(10) A very important statement is made by Babes, but upon this he lays no stress. It is that he has found some of the varieties outside the body (in drinking-water), but the typical germ has been found only in the bodies of those dead of typhoid fever. The facts given in the lecture will probably explain this statement.

(11) Animals inoculated with the atypical germs die more certainly (*i.e.*, a larger proportion of them die) and more rapidly than those inoculated with the typical germ. Exactly the same changes are found after death.

How Babes can conclude, after all this valuable work, that his typical germ is the sole cause of the disease, I cannot understand. It is true that he indirectly implies that other germs might cause the disease, because he constantly speaks of his typical germ as "the typical germ of our cases."

In again concluding, I wish to say that it seems very evident to me that the belief that typhoid fever is due at all times to one and the same germ, which can be recognized to the exclusion of all others by certain peculiarities in staining or in manner of growth must be abandoned.

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* Neither A nor B give off any putrefactive odor, even after the pieces of spleen, described in the lecture, were kept in the incubator for two months.

THE TREATMENT OF TYPHOID FEVER.

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A Lecture delivered at the Post-Graduate Course of the University of Toronto, Dec. 19, 1890.

Gentlemen:

It would seem fitting that the very excellent address upon the etiology of typhoid fever, to which we have listened, should be followed by some remarks upon the treatment of that disease. It was the wish of the University authorities that this part should have been undertaken by one who had spent as much time and energy in ascertaining the best methods of treatment as Prof. Vaughan has spent in the investigation of the origin of this disease. Owing to domestic affliction, the gentleman whom we had hoped to secure to address you upon this subject was prevented from coming, and, at a very late date, it was thought best that I should try to fill the vacancy.

One consideration more than any other caused me to take up this subject rather than let it go by default. During the past three months, while having under my care a very large number of cases of typhoid fever in the Toronto General Hospital, I have become convinced that I at least, and probably many other practitioners as well, have been inclined too much to rely upon a purely expectant plan of treatment, and that we have not employed all the means at our disposal to counteract the effects of this poison upon the human system. When it is stated that in the nine years previous to 1880 there were 27,000 deaths from typhoid in England alone; that in Ontario we have had in the nine years beginning with 1880 4,450 deaths, an average of about 500 each year, and that the mortality from the expectant plan of treatment has been in the neighborhood of 20 per cent. of all cases, you will agree with me that the subject is well worthy of our careful study, and that its importance is a sufficient apology for a lecture upon such a well-worn theme.

The theory of typhoid fever which in the present state of our knowledge seems to be the most probable is that the virus, in the form of bacteria, when swallowed, "is taken up by the lymph follicles of the ileum, and carried to the mesenteric glands, where it may remain for a time in a state of latency. It may then pass into the general circulation and be deposited in the lymphatic organs, including the spleen." After the introduction into the system, the bacteria favor the production of ptomaines, which in time act as poisons on the various tissues. The virus produces rapid hyperplasia, together with other local changes. One of its most constant effects is that upon the heat centre, producing in the majority of cases a high degree of fever.

The increased temperature, again, together with the toxic condition of the blood, is the cause of the most serious lesions found in typhoid cases; the softening of the heart muscle: the fibrous degeneration of muscular tissue; the general febrile consumption of the body; delirium; sleeplessness; and the typhoid conditions, although not altogether the result of high temperature, are largely influenced by it. Moreover, high temperature produces some of the most serious nervous symptoms, acceleration of the pulse, and favors rapid decomposition of the contents of the intestines. The secretion of gastric juice and stomach digestion are not so much influenced by the fever as the secretion of the salivary and pancreatic fluid, hence starchy foods are most likely to be carried through the intestines in an undigested condition and to produce increased irritation. Such is a brief and necessarily incomplete account of the way in which this poison affects the human organism.

Now the question arises, How is it to be combated? The various methods of treatment adopted may conveniently be considered under four heads: the expectant, the antipyretic, the antiseptic, and, lastly, the specific. By the expectant method is meant the placing of the patient in the most favorable position to withstand the ravages of the disease, and to allow the fever to run its course. To accomplish this he is placed in bed in a cool room, to which abundance of fresh air is admitted. The diet is of a liquid character, and such as to be easily digested, being restricted almost altogether to milk, when that form of nourishment is well borne. The skin is kept in a healthy condition by daily sponging with tepid water, and every deviation of the bodily function is at once looked after. Excessive diarrhœa is checked, as it would be in a healthy person; sleeplessness is overcome by the administration of opiates. At the same time the strength is kept up by nourishment, and, if necessary, stimulants are used to support the flagging energies.

This method has been many years in existence, and has still many followers. That it is a great improvement upon the heroic treatment adopted in the early part of this century, and that through it we have been able to study the natural history of the disease, will be agreed to by all. We may also safely assert that in our present state of knowledge no better treatment can be adopted for mild cases. The question however comes up: In addition to these measures, excellent in themselves, and to be adopted in all cases, cannot something further be accomplished in the management of the more severe forms of this disease? The fact stares us in the face that, according to European statistics, under the expectant treatment there is a mortality of about 20 per cent. in all cases. Within the last twenty-five or thirty years, some physicians, recognizing the fact that many of the most deleterious effects upon the human body were the result of high fever, have by various methods attempted to reduce the temperature, and this has led to the antipyretic plan of treatment. There are two ways of carrying out this method: (1) by the application of cold to the body, and (2) by the administration of drugs which are known to have a positive effect in reducing temperature. In the latter part of the last century the method of reducing temperature by the application of cold was adopted to a limited degree by some physicians. In 1821 the Priesnitz mode of treatment was introduced into Europe by an empiric who, although not a scientific man, was largely endowed with shrewd common sense. This treatment by cold packs was for a time very fashionable, and in many forms of disease; in cases of fevers, among others, it seems to have been quite successful.

To Dr. Brand, of Stettin, belongs the honor of first adopting and actively advocating the cold bath treatment for typhoid fever. His earliest contribution upon this subject appeared in 1861, and since that time many articles have appeared from his pen. In the *Wiener Medicinisch Wochenschrift* of 1872 he made the assertion that no patient with typhoid fever will die if this mode of treatment is adopted from the very commencement of the disease, an assertion which certainly appears to be borne out by his statistics of private practice.

This system was introduced into France by Glenard, who when a prisoner in Stettin, during the Franco-Prussian war, noted the excellent results of Brand's method, and who, when he returned home to his native country, introduced it into his own practice. Recently some of the most enthusiastic followers of Brand have been French physicians.

In 1880 Dr. Cayley spoke very favorably of the cold bath treatment in the Croonian lectures of that year. Shortly after 1880 powerful antipyretic drugs were introduced, and have since that time been largely used in the reduction of temperature. Within the last two or three years, however, a marked tendency to return to the cold water treatment in some of its forms has been evinced. There are four or five principal methods of applying cold, viz., sponging, sprinkling, the application of packs wrung out of ice water, coils, the ice pack, and cold baths. Cold sponging has been perhaps more widely adopted in this country than any of the other forms, and has been found, in some instances, a very effectual method of reducing the temperature. In many cases, however, this method has but little effect. The temperature is either but slightly influenced, or if it falls a degree or so, it rises again rapidly. The application of the pack of cold ice cloths, or of ice itself, is a much more effectual measure. Both methods have been largely used in the Toronto General Hospital during the past four or five months. The rule has been, when the temperature rises above 103° to use first the ice water pack, and if that was not successful, the ice pack. They are applied to the lower part of the chest, extending over the whole abdomen. The ice packs are applied to the abdomen alone. In employing this method care should be taken to see that the pack does not remain on after the temperature had fallen below 100°. I have not in any case observed shock to the nervous system, or any other evil effect from the pack when used in this way. In using the ice, I have the nurse remove and reapply it every fifteen or twenty minutes, so as to allow the skin to react. There is one circumstance connected with the application of the ice pack which might be mentioned here, and which will explain the reason for its frequent removal. The integument immediately beneath the pack becomes very cold, the capillaries are contracted, and the blood does not circulate freely through the part, and, consequently, the cooled blood does not readily pass through the system. Of course, in the deeper parts of the abdomen the blood is more readily cooled. As will afterwards be shown, this disadvantage is done away with in the cold bath system by the vigorous rubbing of the surface of the body. We have also thought it necessary to avoid all wetting of the bed, which can only be done by the free use of the McIntosh cloth.

I have also seen good effects from the use of the ice water coil or pad when placed over the chest and abdomen. The latter has the advantage of dryness and consequent comfort. The treatment by cold baths I assisted in carrying out under the direction of my senior physician when a member of the medical staff of the German army in the campaign of 1870-71. It was then introduced as a regular mode of treatment of typhoid in many divisions of the army. The statistics, by the way, of the Franco-German war are most strikingly in favor of the cold bath treatment. Among the cases treated by the ordinary methods then in vogue, there was a mortality of from 20 to 40 per cent., while among those treated by Brand's system, there was a mortality of from 4 to 12 per cent. Owing partly to the introduction of antipyretic drugs, and partly to the great difficulty of introducing such a heroic form of treatment, I have not since made use of it.

It is a plan which cannot easily be carried out in private practice, and even in hospitals the amount of work given to the attendants is often greater than can be accomplished. A bath of the ordinary size, which will allow the patient to be fully stretched out, and which can be purchased for eight or ten dollars, is all that is necessary. It should be placed at the side of the bed and filled with water at a temperature of from 65 to 70°. A sheet may be stretched over the top of the bath, and the patient placed upon it, and slowly allowed to sink down into the water. In the New York and Bellevue Hospitals, where, owing to the courtesy of Dr. Peabody, I have recently seen the method employed, the sheet was dispensed with, and the patient taken by attendants and at once put in the water. A half-ounce of whiskey is given to the patient before the bath, and hot milk is freely administered when he is taken out. The time for the bath varies from seven to fifteen minutes, according to the effect produced. While in the bath the patient is vigorously rubbed by two strong men, and ice water is occasionally poured on the head by a third attendant. The temperature generally falls for half an hour or so after removal from the bath, sometimes to a point below normal. It is better that the patient who takes a bath for the first time should not remain in longer than seven or ten minutes, as some are much more easily affected than others. After the bath the patient remains in bed until the temperature again rises to 103°, when another is given. Rigors of a severe character often attack patients after the bath. In all cases there is more or less shivering. These symptoms do not appear to indicate any grave constitutional disturbance. The baths may be repeated eight times in the twenty-four hours, if necessary, but not oftener. Many cases require this number at first to keep the temperature down, but in a few days a less number is needed.

The immediate effect of the bath is to quicken the pulse, which, however, soon becomes slower and fuller. Many patients who dread this mode of treatment at first, will soon ask for the baths on account of the comfort they feel from them.

Baths are contra-indicated in fleshy people with weak hearts or a tendency to bronchitis. They are also contra-indicated in cases of intestinal hemorrhage, peritonitis, or threatened perforation, because in such condition it is necessary to keep the patient as quiet as possible. It was at one time supposed that the baths produced a tendency to intestinal hemorrhage by congesting the mucous membrane of the bowel. Experiments upon the lower animals, however, have proved that immersion in cold water blanches the mucous membrane of the intestines, and could not therefore increase the danger of hemorrhage.

Baths are also contra-indicated in nervous cases, when they are much dreaded.

The pneumonia and bronchitis which directly result from the typhoid fever do not contra-indicate the use of baths; in fact the benefit obtained in such cases has been most marked. In cases also in which the pulse is rapid, and there is a marked typhoid condition, a cold bath has often a very bracing effect.

The statistics obtained since the introduction of this plan of treatment are as follows, quoted largely from Dr. Cayley's Croonian lectures:

In Basle, under the expectant treatment, there were out of 1710 patients 469 deaths, a rate of 27.3 per cent. Under a partial antipyretic treatment there were out of 982 cases 159 deaths, a rate of 16.2 per cent. Under the cold bath treatment, out of 1483 patients there were 130 deaths, a rate of 8.8 per cent.

IN KIEL—	No. of Patients.	Deaths	Per cent.
Under expectant treatment	330	51	15.4
Under cold baths - - -	160	5	3.1

IN STETTIN—	No. of Patients.	Deaths.	Per cent.
Expectant treatment - - -	1591	405	25.6
Cold baths - - - - - -	121	5	4

PERCENTAGE OF DEATHS—	Before.	After
Bartels and Jurgensen - - -	15.40	3 10
Liebermeister and Hagenboch -	16.10	8.80
Pfeufer and Lindwurm - - -	13 20	5.40
Hospital in Heidhausen - -	13.50	4.80

MORTALITY IN THE PRUSSIAN ARMY, UNDER COLD BATH TREATMENT,

According to the years, from 1874 to 1880.

Year.	Patients.	Deaths.	Per cent.
1874	2735	329	12.0
1875	3620	408	10.9
1876	2747	298	10.8
1877	2081	206	9.8
1878	2112	190	8.9
1879	1741	163	9.4
1880	2534	226	8.9

The previous mortality under ordinary treatment had been 25 per cent. This table also shows a gradually diminishing rate of mortality year by year, a fact which is probably due to greater attention to detail in carrying out the system. This, like any other active form of treatment, must be conducted with care and skill if good results are expected, and if carelessly carried out much injury may ensue.

Brand's latest statistics give the following percentage of mortality:

Cases in family practice	1.0 per cent.
Military hospitals - -	3.4 ,,
Consultation cases - -	3.4 ,,
Civil hospital - - -	5.0 ,,

The general result shows that out of 8141 patients treated by cold baths there were but 680 deaths, a rate of 7.4 per cent., while under all other modes of treatment, including other forms of antipyretics, the rate has been over 16 per cent.

It is then conclusively shown, so far as it can be by statistics, that the mortality under the cold bath treatment is lower than under any other method, and much lower than under the expectant plan.

The vigorous rubbing constantly kept up by the attendants is a very important element in the process, and that thereby the bath not only lowered the temperature, but acts also as a general tonic to the nervous system. The rubbing at the same time prevents the contraction of the capillaries of the skin, thus improving the circulation and allowing the cooled blood to go to all parts of the system. If the temperature can be sufficiently reduced by the application of the cold pack, and remains so for some hours, there is in my opinion no necessity for the cold bath. There are, however, cases in which even the pack or cold coil will not reduce the temperature more than a degree or so, and when these are removed the tempera ure rises again rapidly. I have had two or three such cases in the hospital recently, in which I would now confidently advise the use of the cold bath. In such severe cases the baths should be given early in the disease, so that the patient may be guarded against the effects of a continued high temperature. One great disadvantage has already been mentioned, they cannot be easily given in private practice, as at least three attendants are required, as well as the surrounding convenience for giving them.

Tripier and Bouvret state many instances where the treatment was carried out in villages and country districts near Lyons. In one village there were twenty cases of typhoid; the first nine were treated on the expectant plan, and of these four died; the other eleven were treated by cold baths, and of these none died. In another town—St. Germain—there were 41 cases treated by cold baths, with no deaths. Taking all the recorded cases which occurred in the neighborhood of Lyons under the cold bath treatment, there was a mortality of 3.62 per cent.

Another drawback to the cold bath treatment is the frequent disturbance of the patient. We have always thought that rest of mind and body was a cardinal point in the management of typhoid, while under this system the patient may be taken out of bed and placed in a bath six or eight times a day. In Berlin this is remedied by placing the patient in a sort of hammock, which can be easily raised and lowered.

At this point I would like to refer to the statistics of our own hospital, where the treatment has been largely of an expectant character. The percentage of deaths for the various years is as follows:

Year.	Per cent.	Year.	Per cent.	Year.	Per cent.
1878	20.0	1883	14.4	1888	13.7
1879	13.6	1884	14.0	1889	15.3
1880	19.6	1885	9.6	1890	12.3
1881	12.5	1886	10.8		
1882	15.7	1887	14.4		

The average percentage of deaths, 13.4. Total number of cases, 1381; deaths, 186; per cent., 13.4; died within six days of admission and therefore not influenced by hospital treatment, 64; deducting this number from the total we have an average percentage of 9.2. It will thus be seen that the statistics in our own hospital, under the ordinary form of treatment, have been very much better than the continental hospitals under the same system, and almost as good as some of them under the cold bath treatment. It would appear from this that we have here a milder class of cases, which do not require the cold bath treatment to the same extent.

In order to form an estimate of the number of cases having a persistently high temperature, I had an examination made of a hundred temperature charts, with the following results: One reached 106; ten, 105; thirty-four, 104; thirty-eight, 103. The one whose temperature rose to 106 reached this point but once; the ten reached the temperature of 105 but once; of the thirty-four whose temperature rose to 104, in twenty-six this occurred but once, the remainder—two, three, or four times. Of the whole number of cases, in at least forty-five the temperature rose to 103 more than twice.

From my experience in the hospital I do not think that in more than ten cases out of the hundred there would exist a continuous high temperature that could not be controlled by cold sponging, cold pack or coil, and that, therefore, there would not be a larger number of cases requiring the cold bath.

The principal drugs which have been recommended for internal administration to reduce temperature are quinine, digitalis, salicylate of soda, antipyrin, antifebrin, and phenacetin. Quinine has a very uncertain action, often even in large doses it will not reduce the temperature to any great degree. The cerebral symptoms produced by large doses, as well as the temporary deafness, are unpleasant. Salicylate of soda has a more certain action, and has perhaps been too much overlooked since the advent of newer and more rapidly acting drugs. It is doubtful if in moderate doses it has any depressing effect upon the circulation, and it has the advantage of being also an antiseptic agent. The lessening of the heart pulsation after the administration of the salicylates is probably due to the lowering of the temperature rather than to the direct effect of the drug; it does, however, depress the motor nerve centres. Digitalis was at one time thought to be a very effective antipyretic, but its action upon the heart, particularly in some forms of degeneration, is such that sufficiently large doses to produce an effect on the temperature cannot be given. Antipyrin and antifebrin have been for the past five or six years the most extensively used drugs for antipyretic purposes in typhoid fever. Antipyrin is a very effective agent, lowering the temperature in fever often from two to twenty hours at a time. It has the disadvantage, however, of producing in some patients alarming symptoms of collapse when given even in moderate doses. This appears to be on account of an individual idiosyncrasy which cannot be made out previous to the administration of the drug. I well remember a case illustrating this point which occurred in the hospital a few years ago. The patient, a strong, healthy girl, was suffering from an average attack of fever. Antipyrin was given in two ten grain doses. The temperature rapidly fell below normal, and the pulse, which had previously been under 100, went up to over 120 in the minute; it was at the same time weak and compressible, and the patient went into a state of partial collapse, from which she could only be rallied after some hours.

I was shortly afterwards called to see a case in which a somewhat similar effect was produced by a 15 gr. dose of antifebrin. I am confident that I have observed in several cases a distinctly lower power of resistance on the part of the system to the inroads of the disease, and that patients have not recovered so rapidly or satisfactorily after the administration of antipyrin. The theory that these drugs diminish the power of oxidation in the system would also be an argument against their use.

Taking everything into consideration, I do not think Dr. H. C. Wood speaks too strongly when he says: "I believe that cold baths are much safer and more efficient than are antipyretic drugs." When we consider, then, the treacherous character of such antipyretics as antipyrin and antifebrin, that in some cases they do not succeed in reducing the temperature, that they have a depressing effect upon the system which should in every way be supported to overcome an exhausting disease, and when, moreover, we have a much more certain, a much safer, and a much more controllable method of reducing the temperature by the application of cold, it is questionable if we are justified in continuing the use of such drugs for antipyretic purposes. It must, of course, be understood that when there are no means available for the proper application of cold, it is better to give these drugs than to allow the patient to suffer from the effects of a continuously high temperature. A case came under my observation not long ago, in which the temperature ranged between 103 and 105, and the administration of antifebrin and phenacetin in large doses had no effect, nor had the cold coil. The cold baths were ordered, and the temperature was effectually reduced, and the patient recovered.

Under the third head, we shall now go on to speak of the antiseptic method. Little doubt can exist in the minds of physicians that typhoid fever is a germ disease, and after the excellent demonstration of Prof. Vaughan, we at least who have had the privilege of seeing and hearing, must be convinced of that fact. It is almost certain that in most cases the poison is swallowed and taken up by the lymph follicles, and, moreover, that the poison exists in its greatest intensity in the small intestine. At the same time, owing to the high temperature, decomposition of the intestinal contents takes place more rapidly in the intestine. If, then, we can by any means render the contents of the intestines aseptic, we prevent the inroads of poisonous matter into the system; in fact, we attack the enemy while yet in his camp, and place the patient in a safer and better condition. Such agents have been largely used by French physicians, and statistics would appear to demonstrate their usefulness.

In the Toronto General Hospital, we have used such agents as thymol and bismuth, salicylate and B. naphthol. My colleague, Dr. McPhedran, who used the thymol, is of opinion that the general effect has been favorable: the temperature did not rise so high, and the constitutional disturbance was generally less than in cases where the drug was not given; the tympanites was markedly less. In some cases the remedy had to be discontinued, as it disagreed with the stomach. I may make the same assertion with regard to the salicylate of bismuth. The reason for my choosing that drug was (1) its acceptability to the stomach, and (2) that on account of its only partial solubility, it would be more likely to pass into the small intestine. It was given in five grain doses every four hours. I found it to have a decidedly antipyretic effect, as shown by some of the temperature charts. In some cases the temperature fell two or three degrees, and did not rise again throughout the whole course of the disease. I did not notice any unfavorable action upon the heart, and in only one or two cases was the stomach affected.

The antiseptic agents which have been used are naphthaline, B. naphthol, thymol, iodoform, and bismuth salicylate. According to Bouchard, naphthaline, salicylate of bismuth, and iodoform, are more or less absorbed into the system, and cannot therefore be used in sufficiently large doses without producing poisonous effects. B. naphthol is not so absorbed, and passing through the intestine unchanged produces an aseptic condition of the contents.

Dr. Clark, in a recent number of the London *Practitioner*, gives his experience in the use of this drug; he gave it according to the following formula: B. naphthol, grs. xx.; tinct. aurantii, ʒii.; syr. lemon, ℥p.; muc. acacia, ℥iii.; aqua ad., ℥vi; ℥i. to be given three times a day. After the administration of this remedy the stools became much less offensive, and the cases seemed to run a milder course; convalescence, too, was more rapid. The same objection, however, was noticed in the use of this remedy as with thymol, viz., that it caused in some cases a derangement of the stomach. Recently B. naphthol in combination with salicylate of bismuth has been given to patients in the hospital.

There is a great future for the antiseptic method, but we have yet to find an agent which, while effective in destroying the germs in the intestines, will also be soluble and innocuous to the mucous membranes of the alimentary tract.

The treatment of typhoid fever by specifics has been taken up at different times with varying success. Quinine, calomel, and iodine, are the principal remedies used under this head.

It is now generally admitted that quinine has no specific action in this disease, except malarial. Calomel probably acts on account of its antiseptic qualities as well as by any specific effect. Liebermeister gave iodine, in the form of iodide of potassium, to the extent of from 20 grains to a drachm in the twenty-four hours. The drug did not produce any immediate effect whatever, either upon the pulse or temperature, but, as will be seen by the accompanying tables, the mortality was much diminished in those who had been put under this treatment. He also gave calomel to a number of patients in 7½ gr. doses three or four times a day. In these there was also a considerable lessening of the mortality. Out of 839 cases treated by Liebermeister, there were

	Patients.	Deaths.	Per cent.
Non-specifically treated ..	377	69	18.3
Treated by calomel	223	26	11.7
Treated by potass. iodide ..	239	35	14.6

Leaving out all cases which died within six days after admission, Liebermeister had the following results:

	Patients.	Deaths.	Per cent.
Non-specifically treated ..	355	47	13.2
Treated by calomel	216	19	8.8
Treated by potass. iodide ..	229	25	10.9

These statistics would appear to show a decided benefit from the use of the drugs.

I am quite convinced that we are on the eve of great discoveries in medicine, and these will be on the line of specifics. The treatment of tuberculosis recently established by Prof. Koch, a discovery of which we of this province (owing to the generosity of our Vice-Chancellor) will shortly reap the full benefit, marks an event in therapeutics. It is our hope and expectation that Prof. Vaughan, or some other scientist who has been studying the etiology of this disease, will soon be able to give to the profession a remedy which, when hypodermically injected, will either prevent the multiplication of germs or destroy those which already exist.

Stimulants are recommended by some authorities from the commencement of the fever, on account of the antipyretic action of the alcohol, and its supplying material for ready oxidation. I have not been in the habit of giving it until the latter stages of the disease, more particularly if the patient is suffering from septic fever, the result of absorption of poisonous matter from the intestines. In such a condition very large quantities of alcohol may be given with benefit. Recently a patient whom I saw in consultation rallied after the pulse became almost imperceptible at the wrist, and he was supposed to be *in articulo mortis.* An ounce of alcohol, diluted with water, was given every hour, together with ammonia and digitalis. The patient made a good recovery.

Milk, pure and simple, is the best form of diet for typhoid patients. It should be given in moderate quantities, frequently repeated, so as to prevent the formation of dense curds in the stomach. The addition of lime water or beef peptonoids is often an advantage, and from the favorable reports we have of the use of sterilized milk in the diarrhœas of children, that form ought to be specially adapted for typhoid cases. Many patients who at first think they cannot take milk will after a time find no difficulty in digesting it. Care must be taken not to give nourishment in larger quantities than the stomach will digest, or than is necessary to support the system. If too much is given, the intestines become loaded with matter undergoing decomposition, irritation follows, and increase of tympanites, both unfavorable to the progress of the disease in the bowels. In a few cases milk cannot be given, as it will not agree with the stomach; then animal broths, beef peptonoids, etc., must be substituted.

The question of when to resume the use of solid food is often one of importance. As a general rule, we allow solid food in the hospital after the temperature has been normal for a week. In severe cases, however, when there has been serious bowel trouble, a longer time is allowed to elapse before the resumption of solid food. Boiled custard, lightly boiled eggs, and scraped beef are first given, to be afterwards followed by the starchy foods. There are many cases in which we could commence the use of solid food earlier if we were only certain of the condition of the intestines.

A case is mentioned by Dr. Cayley of a boy who was taken from the hospital by his mother because, according to her ideas, they were starving him. She gave him ordinary food, and, contrary to expectation, the boy became strong very much more rapidly than if he had remained in the hospital. A similar case occurred in our own hospital, but with the opposite result. A boy was taken out when convalescent and given ordinary solid food; perforation took place and death ensued.

Notwithstanding the fact that we have no accurate idea of the condition of the bowels, and while believing that it is by far the best policy to err upon the safe side, it has occurred to me that in some cases we allow the patient to suffer by withholding solid food for too long a time. We might try earlier by giving small quantities of such food as will be digested in the stomach: scraped beef, poached eggs, etc., to test the capacity for ordinary food. I am convinced that we too often ascribe the blame for relapses to errors in diet. We find quite as many relapses where there has been no changes made in the food; and, on the other hand, grave errors have been committed which have not been followed by relapse. We must recognize the fact that relapses are, under any circumstances, frequent in typhoid fever.

Tympanites, which in the earlier part is due to paralysis of the muscular coat of the intestine, together with increased decomposition, and in the latter stages is due in addition to intestinal lesion and peritonitis, is, when extensive, a very grave symptom; it impairs the respiratory and cardiac movements, and favors the occurrence of hemorrhage. The local application of cold will frequently relieve it when the temperature is high. The internal administration of turpentine, or one of the antiseptics already mentioned, may be necessary.

The introduction of a rectal tube was found of very great service in two or three cases in the hospital this fall. The tube is of more use when there is little or no peritonitis. When all other means fail and the symptoms are urgent, it may be necessary to pierce the bowel with an aspiration needle.

Flint recommends the administration of frequent drachm doses of sulphate of magnesia when other remedies are found to be useless. The treatment adopted in our hospital for hemorrhage is the internal use of turpentine and opium. The latter drug has almost a specific action when there is extensive sloughing of the intestinal walls, acting in the same way as in sloughing phagadæna. For the headache, which is sometimes a very troublesome symptom during the first week, I have found phenacetin to be the best remedy.

According to statistics, relapses occur in from 1.4 to 11 per cent. of cases. They are more frequent when any form of antipyretic treatment has been adopted, and this is generally admitted to be one of the drawbacks to such measures. As a rule, however, relapses are not fatal, and as the general results are so much better under antipyretic treatment, this drawback is not of any great account; besides, it may possibly be that the cases which have relapses under antipyretic treatment, would probably have died under expectant measures. A relapse is certainly to be preferred to a fatal issue.

I would not wish to conclude without bearing testimony to the great value of skilled nursing in the management of typhoid. To give the patient the best chance for recovery it is necessary that the details should be rigidly carried out by one who has a thorough knowledge of the disease. The statistics of our own hospital give ample proof of the value of trained nursing. Although the training school for nurses was opened in 1882, it was not until 1885 that it was put in thorough working order, so that the benefit to the patient was fully realized. It will be seen by referring to the percentage of mortality, and comparing the seven years which preceded 1885 with the six following years, that there was a gain of between three and four per cent. In other words, about four typhoid patients out of every hundred have been saved apparently by the introduction of a better system of nursing.

I have thus attempted to give my views on the treatment of typhoid fever, having gone over those points upon which I had some personal experience, leaving out such as are of lesser consequence, and upon which I have no personal views to offer. I will conclude with the hope that before many years we shall have such an improved sanitary condition in Toronto that an epidemic such as we have had this year may never reoccur. I also hope that for the cases which will no doubt occur occasionally we will have a form of specific treatment which shall far exceed any yet known.

THE PRESENT LIMITATIONS OF SPINAL SURGERY.

By ROBERT ABBE, M.D., NEW YORK.

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I VENTURE to present to your attention a subject not too old to be hackneyed, nor too new, as yet, to have been anywhere nearly exhausted. The surgeons of the past have uniformly insisted upon the most rigid respect for interference with the spinal cord in pathological conditions. Occasional attempts have been made to restore its integrity when it was supposed to be pressed upon in fractures; but so much disaster has followed upon attempted interference, that no progress can be said to have been made until the last five years. At present we find ourselves in a position to endorse surgical approach to the spinal cord under very many circumstances. What amount of benefit will ensue from such interference, it remains for additional experience yet to prove. We shall not enter into a rigid survey of the history of the surgery of the spine; for this has already been well done by Dr. White, whose account can be read in the *Annals of Surgery* for July, 1889. Since that date, somewhat more has been added that gives additional interest to the subject, and outlines, in a measure, the direction of future investigation. At present I only go so far as to call your attention to the now existing limitations of the work, and to illustrate the subject by histories of typical cases.

The general surgeon has dismissed from his mind the question of suspension in order to relieve certain "medical" lesions of the spine, as in locomotor ataxia. This mode of procedure has had a fair and extensive trial, which has gone to show that its utility is too slight to be taken seriously into account; and it is now practically abandoned. The more truly surgical operations upon the spine include such interference as will deal with cases of spina bifida, with caries from Potts' disease, with tumors of the spinal cord, with intra-dural adhesions from myelitis, and with fractures of the spine. We are also in a position to take up the question of the section of the spinal nerve-roots.

Under these six headings let us consider the last views sustained by recent practice. It will be seen that in dealing with the condition of spina bifida, we have at once to attack one of the most delicate forms of spinal work. We have immediately to invade the serous cavity of the spine; to subject the patient to the great bugbear of the past surgery of the cord, spinal meningitis. But it is now evident that with the modern precaution of exclusion of infection, we practically exclude this risk. The purely surgical method of dealing with spina bifida cases, which has been outlined and defended so well by Mr. Mayo Robson, presents the best possible resort of the surgeon of to-day. The bold and complete excision of the sac, with suturing of the membranes composing it, has given results superior to anything yet known. In spite of the fact that the special committee of the Clinical Society of London some years ago announced, as the results of their laborious research, that by far the best and safest method of curing the condition was by the injection of Morton's iodo glycerin solution—by which sixty per cent. of the patients operated on were "cured"—there comes to those who use it a far poorer record! Cases of spina bifida differ so much in their interior structure, as regards the distribution of the spinal nerves, that it becomes difficult to know, before injection, whether the nerves themselves are not in the sac, where they will be bathed by the irritating fluid. On the other hand, the method of Mayo Robson enables one at once to inspect the interior and to make such disposition of the nerve-ends as may seem surgically best. After having twice used the injection method, with results I was forced to regret—convulsions having ensued, and death—I have lately had an opportunity of removing a very large spina bifida, in which about one-third of the cauda equina traversed the sac, passing in the median plane to the opposite wall. This I severed and laid back in the groove of the divided spine; then I excised the entire sac. This case had been subjected to repeated aspirations by the physician in charge before I saw it, and had an inflamed condition of the sac, with general fever; temperature 105; and death seemed impending when I operated. The temperature declined immediately after operation, and the spina bifida was cured. At the end of three weeks the child, being out of my care, fell into some disorders of nutrition and died suddenly. An autopsy was obtained, and the site of the tumor found to be perfectly healed. The ease, the satisfaction, and the safety of the operation, renders it, to my mind, the only rational treatment. As regards the loss of spinal fluid involved by entering the sac, it entails no danger to the child, provided the patient be kept in a horizontal posture or with the hips elevated. No drainage can then take place from the cranial cavity or the upper spine. The child does not suffer convulsions; and the operation is quite as simple as when we operate elsewhere on serous cavities. Fine catgut suturing of the serous lining, not quite underlying the line of skin incision, renders good primary union, with a strong covering, quite certain.

Robson reported, among others, one recovery in a case where the sac was inflamed, and its contents slightly turbid.

The question of interference in caries from Potts' disease hinges upon two conditions: First, in the early stage, before extensive destruction of bone has occurred, an endeavor may be made to abbreviate the course of the disease by curetting, and treating the lesion precisely as we deal with tubercular lesions elsewhere. Second, there are cases where paraplegia has followed upon angular curvature from destruction of the bones, or from interspinal pressure from neoplasms incident to the disease. Surgical museums everywhere attest the power of Nature, by building up ample osteoplastic involucra for the carious vertebræ, to handle the question of surgical repair quite as well as the doctor. What she wants in the way of help, however, is that we should rid her of the burden of the tubercular infection. It seems as if we had almost in view that relief, from Koch's fluid. But as the use of this is not yet established, we can fairly say that our best method of relieving these carious conditions is through the free use of the curette, the injection of iodoform; and if there be bad sinuses, the use of peroxide of hydrogen. It is not often that cases of caries of the spine are early operated on; but I conceive that the course of the disease will, in some cases, be rendered more brief by direct local treatment. This was one of the earliest dreams of Lister, after initiating antiseptic surgery, but a disappointing one. The use of iodoform, however, has assisted materially in justifying this expectation. The disease rarely begins in the arches of the vertebræ; more often in the vertebral bodies. Kraske, of Freiburg, regards this portion as too inaccessible to be operated on successfully. I have myself operated in one case which well illustrated the ease with which the bodies of the vertebræ may be approached. My method was illustrated briefly as follows: The patient, a young man of twenty, had a history of pleurisy, *fistula in ano*, and slight phthisical changes in the apex of one lung, a history extending through two years prior to the development of a lumbar abscess over the iliac crest. This last was being freely discharged through three sinuses along the iliac crest. A probe passed far up the spine to the last dorsal vertebra. There was no deformity, and little pain. An incision was made alongside the twelfth dorsal vertebra. The transverse process was carious as well as the body of the bone adjacent; and a curette was readily passed into the body of the vertebra, alongside the spinal dura mater, which was readily pushed aside without injury, and an extensive excavation of diseased bone made, one-half of one vertebral body being thus removed. Apparently healthy bone was left on all sides. The entire pus channel was curetted, and douched with sublimate solution, then with iodoform in ether. In six weeks the patient left the hospital, having, at the time, a very slight discharge from one small sinus. Prior to operation he had had three sinuses, an infected tubercular pus-tract, a foot long altogether, leading to the carious cavity. His condition had been one of severe drain, with hectic. His improvement was rapid and satisfactory, the *rationalé* being that he had ceased to auto-infect his system from an extensive suppurating tubercular pus-channel. I believe the greater part of the discharge in these cases comes from the granulation lining the sinuses.

The second class of Potts' disease lesions that we are called on to interfere with is that of paraplegia, the sequel to chronic caries. While these cases are sometimes due to secondary myelitic changes, they are usually produced by, and due to, extra-dural pressure of tubercular neoplastic tissue. The simplicity of the surgical procedure needed to effect the relief of this extra-dural pressure would make it seem desirable that every such case should be speedily operated upon, fearing permanent destruction of the cord if the condition was allowed to run on. But it has been shown, notably by a review of cases by Dr. V. P. Gibney, of New York, that the majority of pressure-paraplegia cases from Potts' disease recover in due time by natural processes. Therefore the surgeon's hand is stayed oftentimes. Such authorities as Israel, Bergmann, and Kraske, discourage interference, concluding that, because the disease cannot be eradicated, it is useless to interfere. My own feeling would be in favor of interference in the majority of cases, believing, as I do, that thereby the cord may be oftentimes preserved, and the course of the disease simplified.

The orthopedic surgeon is generally rather conservative, and regards the operating surgeon as a man suffering from a continuous and severe surgical fever.

My friend Dr. Gerster operated last year on an old vertebral Potts' paraplegia, and found a subdural abscess with caseous masses, which he curetted away. The patient made a comparatively rapid and entire recovery.

Dr. Wyeth had a somewhat similar experience. The treatment of cold abscesses from carious spines, as well as of the parts about the bone immediately diseased, by the method of Brun (injecting one drachm of twenty per cent. solution of iodoform in olive oil every two or three weeks) deserves attention. It is endorsed by the experience of Krause and Trendellenberg. My own high esteem for iodoform in these cases leads me to endorse it also.

One of the most terrible accidents that man is liable to is a fractured spine—an accident reducing the human body, in the twinkling of an eye, to a state of absolute inaction, indifference, and paralysis. The incidents of the fracture are: the shock; the absolute loss of motion and sensation; and the occasional onset of great pain. The associated train of symptoms will perhaps be as well illustrated by a narration of two or three interesting cases in which I have operated within the last two years as by any general considerations that I might offer. An extensive study of injuries to the spinal cord, made by Dr. William Thorburn, of Manchester, England, has led to the observation that out of twenty-one cases of injury to the cervical region, without fracture of the bone, at least five or six were cases of hemorrhage within the vertebral canal; and that the symptoms, referable as they are to a considerable portion of the spine rather than to one or two nerve-roots, have caused these cases to be usually regarded as fractures, and, later, classified under "concussion." They are not nearly so fatal as fractures and dislocations. Nearly all of the fracture-dislocations of the cervical spine are fatal, whereas the hemorrhages into this portion of the spinal canal end in recovery, though the patients suffer temporarily from paralysis in the limbs and trunk, lasting for from two to three weeks. The hemorrhage usually occurs within the substance of the cord, a so-called hæmatomyelia. The symptoms produced by this intra-medullary hemorrhage are either such as arise from a destructive or such as are due to a compressing lesion; just as in the case of hemorrhage into the cerebral tissue. Resulting from a destructive lesion we find atrophic paralysis, and perhaps persistent anæsthesia in some parts of the nerve-supply to the body. The hemorrhage compressing the cord, either from within or without, may cause more or less complete paralysis and anæsthesia in parts below its level, together with retention of urine and fæces, priapism, contraction of the pupil, and other symptoms. These subside, however, leaving only some spastic disturbances in the limbs. The intra-medullary hemorrhages are the result either of concussion or of fracture dislocation. Below the cervical vertebræ, traumatic hemorrhage alone into the substance of the cord is rare. The fractures about the roots of the brachial plexus, the roots namely of the sixth, seventh, and eighth cervical nerves, have been of special interest for study. Mr. Thorburn found that it was possible to locate exactly the seat of the fractures in this region by a study of the anæsthetic areas in the arm, which indicate with precision that particular root of the brachial plexus which is involved in the damage to a vertebra. The brachial plexus being composed of the fifth, sixth, seventh, and eighth cervical, and the first dorsal, nerves, it has been possible to define the sensory supply of the fifth root as covering the deltoid muscle, and the outer side of the arm and forearm; and that of the eighth cervical and first dorsal as including the little finger and the inner side of the hand, forearm, and arm the remaining roots providing for the intermediate parts of the limb. The fifth cervical root has a definite muscular distribution, constituting the so-called "fifth root group," and consisting of the deltoid, supra and infra spinatus, and teres minor. Consequently, we have only to observe the exact areas of anæsthesia and motor paralysis to know the precise vertebra that is fractured. The completeness of the anæsthesia and paralysis gives us no absolute information at first as to whether the cord itself is irreparably damaged, or whether simply hemorrhage has occurred.

Injuries of the cord from fracture in the dorsal region are easily defined by the anæsthetic area resulting. The fractured vertebra lies about three inches higher than the line of well-marked anæsthesia, owing to the roots leaving the cord within the vertebral canal at a point two inches above where they make their exit from the canal. Lying beside the cord, therefore, in the canal, there are roots that have left it some distance above, and are descending to their foramina of exit. A crushing wound, consequently, will sometimes involve these strands also, as well as the cord itself; though, as a rule, the relative firmness of the nerve-tissue is so much greater than that of the white substance of the cord that the former is apt to escape injury, quite. (Specimen shown.) Owing to the fact that the medullary substance terminates at the first lumbar vertebra in the firm cauda equina, an injury to the spine below this point is less likely than anywhere else to destroy the nerve-tissue. And it is in just these cases that more may be hoped for from operations to remove pressure than in lesions higher up. I have lately seen, with Dr. Lloyd, of New York, such a case, in which I assisted him to operate, and with great relief to the man. A heavy gas-generator had fallen upon the back of this patient while he was down on his hands and knees. His third lumbar spine was crushed in. He immediately suffered complete paralysis of his right leg. He was left, after some time had passed, with a pain in his back and retention of urine, the paralysis slightly improving. But subsequently, six months after the accident, that is, he was still paralysed in his right leg, and anæsthetic over a limited area of the buttocks and thighs, and in the right lower leg and foot. (Blackboard illustration.) We operated in July last, removing the second, third, and fourth lumbar spines, and removing the bones of the three corresponding arches. We found the dura apparently uninjured, and satisfied ourselves with the relief of the pressure made by the crushed-in arch. The patient gained, from the day of the operation onward. His anæsthesia has steadily diminished, until now he lacks sensation in but a small area about the buttock, and along what is merely a line down the leg. He walks with one cane; has little or no pain; and has resumed his work. Thorburn mentions several corresponding cases —though they were not operated on.

It has been more and more clearly demonstrated, year by year, that cases of fracture-paraplegia in the course of the dorsal vertebræ are, in the great majority of cases, associated with extensive damage, and usually with complete severance of the cord itself. The mechanism by which this is effected is the violence sustained in the so-called fracture-dislocation, *i.e.*, dislocation of one vertebral body from its neighbor, together with fracture of some resisting portion of the arch. It is a singular fact that, in the larger number of such accidents, the bodies of the vertebræ are spontaneously replaced, so that the patency of the vertebral canal is preserved! It is not usual to find much permanent encroachment of the broken arch inward upon the damaged cord. The cord is pulpified, and undergoes subsequent degeneration. The portion cut off below continues to exercise its proper reflex acts and functions; hence the response of muscles getting their nerve-supply from it to electric stimulation and to reflex irritation at times; and hence, too, the comparatively plump and persistent condition of the lower extremities. Thus it will be seen that we have two conditions in fracture of the spine producing paraplegia and anæsthesia: one hemorrhage into and about the cord, the other the total transverse destruction of it; and it becomes a matter of importance to determine, if possible, which has been the lesion. If it be hemorrhage only, there will be an early restoration of function in from a few days to a few weeks, which will settle the question. These cases are usually called "shock." If the cord be pulpified, it will never serve its purpose again. Bastian maintains, and his views have corroboration from such cases as I have seen, that there is loss of all tendon reflexes in the paralyzed parts where the cord is severed. This gives us a satisfactory symptom. Let me illustrate these conditions by referring to a case about which I was consulted last week.

Miss N., ten years of age, fell from an apple tree in July, 1889, while breaking a stick over her knee. She was picked up paralysed from the neck downwards, and in pain. The skin over the upper part of her body was exquisitely sensitive to the touch. There was paralysis of the rectum and bladder. The striking features of the case were: (1) A return of power and sensation in both arms and down to the line of the nipples, while the lower extremities remained paralysed. This condition can be explained only by there having occurred a hemorrhage into the cord in the cervical region, and a severance of the cord by a fracture-dislocation at the fifth dorsal vertebra. (2) There continued for some weeks a sweating of the upper part of the body with hyperæsthesia, both due to irritation of the clot in the cord. (3) There remains a persistent objective sense of prickling in the lower extremities. This, unfortunately, cannot be construed as a return of sensory conduction, but merely of irritation at the severed end of the cord. (4) There is an absence of all the tendon reflexes, which is a proof of cord section. (5) There is a notable vaso-motor conduction independent of the cord, manifesting itself as follows: if the rectum or bladder is full, or if the rectum is irritated, or if alcohol is applied to a sore spot over the sacrum, she is at once warned by a feeling of headache and cerebral hyperæmia. (6) There is a peristaltic intestinal reflex if the finger be placed in the rectum, and a stool is regularly induced thereby. From our present knowledge, an operation on her spine would be futile.

The condition of the spinal cord found at operation in fracture paraplegia may be well illustrated by the following cases:

A young man of twenty-seven years had his back broken by falling from a platform twenty feet above the ground, suffering the usual paralysis and accompanying incidents. Complete anæsthesia remained below a line two inches above the crest of the ilium. I operated by my method (to be described further on), and found that the twelfth dorsal vertebræ had been displaced backward, so that the cord was compressed between the arch of the eleventh dorsal above and the upper lip of the body of the twelfth below.

When the arch was removed, the cord was seen to pulsate above but not below the point of pressure. However, it quickly filled out, and there was no surface appearance by which one might say from looking at the unopened dúra that the cord was not normal within. I emphasize this because surgeons occasionally report that the "cord appeared normal" and "they therefore did not open the dura." On slitting up the dura mater for two inches, adhesions of various density were found within, attaching the meninges to the dura, and forming a complete circular dam, shutting off the upper from the lower part of the canal. An ounce of spinal fluid flowed out when the adhesions were broken up. The normal cord ended at the point of injury by a sloping rather than an abrupt edge. A gap of three quarters of an inch intervened, below which the cord again seemed sound. The intervening portion was a mere flattened band of atrophied tissue, with very few white bundles.

The patient had expressed willingness to have resection of the injured portion performed, and suturing of the fresh ends, if that should prove practicable. I found it entirely impossible to approximate the sound cord on either side of the damaged part more than a quarter of an inch, by making traction with tenacula. This patient had primary union of the wound, but gained nothing with respect to the paralysis. Being heard from a year later, he was enjoying the best of health and weighed more than ever, but was obliged to live in a perambulator.

In another case a man of twenty-seven attempted to alight from the rear platform of a rapidly moving car. He was struck and thrown forward, and immediately suffered paralysis at the waist. At operation I removed the arches of the last three dorsal and first lumbar vertebræ. The eleventh dorsal had evidently been fractured, but there was no displacement or pressure. The dura mater was round and full, and as far as its surface showed, nothing wrong would be suspected within it. I slit it up for three inches, letting out some little fluid. Underneath the injured arch a circular dam of lymph was found, cutting off the upper from the lower part of the canal. The white substance of the cord ended abruptly, and was replaced for half an inch by a pinkish-gray substance, the remnant of the membranes.

Just below the lymph dam a congeries of distended vessels occupied the surface of the cord. These emptied themselves perfectly when the lymph adhesions were broken up. Convalesence without fever followed. On the following day his paralysed parts were acutely hyperæsthetic. A touch, or pulling of the hair, of the foot, legs, and thighs, gave acute pain, and caused the legs to jump. There was absolutely no voluntary motion, however. The hyperæsthesia proved to be transient, and up to three months afterward no gain had taken place in muscular action.

To illustrate the hardships of paraplegia cases, the following one is notable. A well-to-do merchant of twenty-seven was thrown from his horse while out riding on his ranch in Montana. His back struck across a stick of wood on the ground, and his body was instantly paralysed below the waist. He endured a sleet and snow storm for a day and a half, lying where he fell. When found he had over-distended bladder. The nearest physician being ninety miles away, he relieved his bladder by straws, the ends of which were rendered smooth by dipping in hot candle-wax. His second mishap was in having both feet burned by the over-zealous use of hot bottles. One month later it was necessary to amputate both legs below the knees. Bed sores subsequently occurred. He afterwards convalesced so far as to enjoy his pastime of riding in a buggy, to the leathern seat of which he was accustomed to be strapped. After unwisely remaining seated thus one day for seven hours, a slough from pressure-gangrene occurred under his buttocks, exposing the tuber ischii. An operation was done to explore the seat of fracture, and offer him some novel methods of relief, but in his bad state it proved too grave for him to rally from, and death saved him from suicide, which he had threatened.

Such cases represent the types of fractured spines. Let us now for a moment examine some typical tumor cases. The famous case of Mr. Horsley, of nearly four years since, is familiar to you all. Its diagnosis rested upon neuralgia of the fifth intercostal nerve. A small oval tumor was found somewhat higher than the apparent exit of the fifth dorsal pair of nerves within the dura, and was removed. The man made a perfect recovery. I have operated on two patients—whose cases have already been put upon record, the first being a tubercular tumor within the vertebral canal but outside the dura, making complete pressure upon the cord for two inches and a half, developing with great rapidity, and including complete paraplegia and exhausting hectic. The man made an absolute recovery after operation for removal of the growth, and is now, at the end of two years, in excellent health.

The second case was that of your townsman, Professor McGregor, whose history in brief was that of strained back two years or more before the onset of his trouble. In July, 1889, he first felt a decided pain in his back, his health began to decline, intestinal action became torpid, urinary retention succeeded, and pain, mostly in his right side, centered in his back. He was at first thought to have muscular rheumatism, and accordingly he endeavored to exercise and work it off. In two weeks he found it difficult to rise and dress; in five weeks he lost his ability to guide his limbs; he walked, pushing a chair. Further efforts to exercise were succeeded by complete paralysis. He pushed a chair before him, but fell upon his side. With indomitable will he made another effort, and fell upon his back then crawled to a sofa, where he lay all night, and subsequently was completely paraplegic and anæsthetic below his waist. His physicians, and Dr. Seguin, of New York, diagnosed pressure-paraplegia either from caries or from tumor. A temporary resort to mechanical support resulted in no gain. As a *dernier resort* he was put under my care for operation. At the time the patient was suffering from albuminuria and fever. Prior to the operation he passed through several attacks of an evidently septic nature, with chills, high temperature, and acute albuminuria. Operation was postponed from time to time from necessity, owing to the condition of the patient. An operation was finally done on April 16th, 1890. It resulted in my finding a sarcoma infiltrating the bone from the base of the spine of the eighth through the arches of the eighth and ninth dorsal, and the body of one vertebra. It also grew between the transverse processes of these vertebræ, and forward between the ribs, beneath the pleura. It occupied the vertebral canal and squeezed the cord flat over the length of an inch. It was entirely curetted away. He improved and bid fair to recover for four days, when obstinate hiccough set in, which grew worse and worse in spite of all measures to check it. It transpired that he had previously been subject to such attacks of vomiting and hiccough. From the fourth to the ninth day, when he died, his stomach was his worst trouble, and from its upset condition his exhaustion resulted.

A letter from Dr. Seguin mentions the ground of his differential diagnosis between myelitis and pressure-paraplegia, "the diagnosis turning upon the presence of fixed pain in one side, and also, with less logical force, upon the comparatively slight anæsthesia at a time when the volitional motor impulses were wholly arrested. A focus of central myelitis in the dorsal region would give rise to a paraplegia with equal sensory and motor symptoms, but with probably greater sensory symptoms, and the side pain would be absent."

We will now speak briefly of an entirely new operation, relieving occasional cases of intractable neuralgia by intradural resection of the posterior roots of the corresponding nerves. It is based upon the fact that this root of each spinal nerve is a purely sensory one. Within the dura it is free for an inch or more, but just outside it enters its ganglion, and unites with the motor root. The possibility of dividing the sensory root on the proximal side of the ganglion was proposed by Dr. Dana, who referred to me for operation the following case:

A middle-aged man, having had exposure to cold two years ago, immediately became the victim of intense neuralgia of his right fore-arm and hand, which subsequently extended to the brachial plexus. He had had his nerve stretched, and finally his arm amputated at the middle of the humerus, by Dr. Bull, of New York. His pain, however, had not abated. He acquired a morphine habit, taking half a grain hourly to subdue his pain. Some months after he came to me for operation. I exposed more than two inches of the cord in the cervical region, the portion corresponding to the roots of origin of the nerves involved. It appeared normal. On the following day, I removed the packing from the wound, and had the man placed in an excellent light with the head lower than the spine. Without anæsthesia I split up the dura mater the full length of the wound, which was a painless procedure, then picked up the roots of three of the brachial nerves, divided them close to the posterior column, and cut out one-fourth of an inch of each root. The dura was then sutured with fine catgut, and primary union resulted. Two ounces of cerebro-spinal fluid escaped without appreciable effect on the patient. His pain entirely changed in character. After eleven days, he sat up and stopped his morphine. The skin of the shoulder and arm remained completely anæsthetic; his morphine habit never returned; his pain somewhat returned, but was never as bad as before. Inasmuch as I did not cut all the roots of the brachial plexus, it is possible that the operation would have been entirely successful had it been a little more thorough.

A second case soon presented itself in a man of forty-five, who, after exposing his arm, when perspiring, at the window of a street-car during a long ride, began to experience neuralgia of his hand. Severe pain persisted, and in the following year Prof. Stinson, of New York, stretched his ulnar nerve, which was subsequently exsected by Dr. Fluhrer, both operations failing to afford the patient relief. Subsequently, Dr. Gerster stretched his brachial plexus at the axilla. However, his pain still continued in a most aggravated form. He acquired a heavy morphine habit. I operated in February, 1889, resecting four of the five sensory roots of the cords of the brachial plexus. For five days he seemed to be much freed from pain. The paroxysms, however, afterward came on, only to a moderate degree, and have up to date never been as severe as before.

The pathologists' report upon the condition of these resected nerve-roots was that they were in a state of inflammation. The future of this operation may be an important one. In a case in which the patient was operated on by Mr. Bennett, of London, four days prior to my first operation, for the relief of obstinate sciatica, and the posterior roots were divided within the dura, a perfect cure resulted up to the twelfth day, when the man died of other causes. This and my two cases are the only ones in which the operation has ever been done.

A most interesting case has lately been published by Drs. Dercum and White, of Philadelphia (*Annals of Surgery*, July, 1890). A man of fifty-five had an attack of what was supposed to be rheumatism, which commenced with shooting pains in arms and shoulders. In three or four days he began to notice a weakness in his extremities, which progressed and ended in complete paralysis a few days later. The reflexes were exaggerated. There was pain over the dorsal vertebræ. Jarring the spine was exquisitely painful.

Dr. White operated, opening the dura and finding an apparently normal cord, adherent by the meshes of the membranes to the dura mater by many fine bands. These were simply broken down and the wound was closed. The patient's acute pain present before the operation entirely disappeared, and never returned. Sensation and power did return, and before one year had elapsed he was walking, and had made an excellent recovery. Whether removal of the irritation due to the presence of lymph bands in connection with the posterior column of the cord, or whether the altered circulation in the operated part resulted in recovery, I cannot say. At all events the case stands unique in its successful issue.

Method of Operating.—I believe the future of successful spinal work depends somewhat upon the technique of our operative method. Heretofore the surgeon has stripped the spines of the attached muscles, severed the interspinous ligaments, and cut away the spines themselves, before dividing the laminæ. This is tedious, and may be bloody. Dawbarn and others have used an H-shaped incision, cutting through one interspinous ligament, and turning up a huge flap of skin and bone and muscle from over the operated part. Compared with the method which I will now detail, these modes of procedure are both crude:

A bold incision a half inch to one side of the spines is to be made, clean and quickly, through the soft parts to the laminæ, parallel with a block of at least five spines. A heavy bent bone-cutting forceps is used to sever the spines at their bases. The interspinous ligament is not cut. A periosteal elevator is now used to scrape the laminæ clean of the adjacent muscles, and the entire block of severed spines is drawn to the opposite side by the aid of broad retractors and periosteum elevators. Thus, without sacrificing any tissue, the entire breadth of the laminæ is thoroughly exposed in a clean wound. To remove the laminæ, bone-cutting rongeurs, a straight and a curved pair, are the only instruments necessary. Their cutting edges should meet at a slight angle, like a flat Gothic arch. In a very few moments an ample display of the spinal cord can be effected, and after the operative work upon it is over, the block of spines, with their muscular attachments intact on one side, falls back into place, and is sutured. Thus there is no weakness of the spinal column remaining from destruction of the interspinous ligament, and no gap to be filled through removal of the spines.

With regard to the possibility of ever replacing a section of damaged cord by sound cord grafted in, fresh from another animal, it would seem as yet a quixotic dream not likely to be realized. Though it is conceivable to my mind that if a liberal substitution of sound cord can be stitched in, in place of a clean cut-out damaged segment of cord, a fair sensory and motor conduction may yet be attainable. The possibility of the successful grafting of a portion of a cat's brain into a dog's brain, as shown by Professor W. G. Thompson, and the recent successful employment of a two-inch piece of the spinal cord of a rabbit to fill a corresponding gap in the median nerve of a man, did away with the theoretical objection that the white medullary substance of the cord and brain would not survive this substitution as successfully as transplanted nerves of the ordinary motor type. The technique of this experiment has yet to be worked out.

CONCLUSIONS: 1. For spina bifida, excision of the sac, after the method of Mayo Robson, is to be advocated.

2. For spinal caries one should only operate where the sinus drainage is exhausting the patient. Then even the bodies of the vertebræ may be curetted, and the sinuses should be abbreviated. Tubercular sinuses should be cleaned up with peroxide of hydrogen and iodoform injections. On cold abscesses Brun's method should be tried.

3. In fracture paraplegia operation should be deferred until the bones have united and hemorrhage has been absorbed. A subjective sense of tingling and pain in the paralysed and anæsthetic limbs is not an evidence of conduction to the cerebrum along the cord, but rather of irritation of the divided stump by the cicatrix, or by bone-spiculæ, and thence a delusive reference to the parts supplied. The only satisfactory proof of total transverse lesion is based on observation of absence of tendon reflexes. Involuntary twitching and jumping is a reflected action having its nervous origin in the distal part of the divided cord. It may exist even years after the injury, and is not to be construed as favorable to ultimate recovery.

4. Little is to be hoped for from operation in cases of total transverse section. If there is pain in the hyperæsthetic zone, it will probably be relieved by breaking up intradural adhesions, and relieving the engorgement of the cicatrix. Nothing more can be expected. Paresis and limited anæsthesia of the lumbar root supplies calls for operation, and this will probably be followed by recovery.

6. Cases of paraplegia and persistent acute pain, warranting a diagnosis of myelitis with local meningitis, should be given a chance of relief, such as that which followed White's operation described above.

7. Simplification of operative methods makes the surgery of the spine a comparatively simple affair.

8. Intradural division of the posterior roots of the brachial or sciatic plexuses for the relief of intractable neuralgias is an operation seemingly justified by the three reported cases. Further experience is needed to prove its title to a place in the list of justifiable operations.

SOME ATYPICAL PHENOMENA IN TYPHOID FEVER.

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A Lecture delivered at the Post-Graduate Course of the University of Toronto, Dec. 19, 1890.

WE occupy our minds so much with the regular and striking features of typhoid fever that we are in danger of overlooking the unusual phenomena that often arise in this disease. Our books continually emphasize the important symptoms, to the exclusion of the rarer ones, that it seems but natural that we should often overlook the latter, especially if we have opportunity of seeing but few cases each season. In so common and important a disease it cannot be amiss, therefore, to direct attention to the more uncommon symptoms, and also to the deviations of some of the important symptoms from the course they are usually supposed to pursue.

I. *The Temperature.*—The description of the temperature of typhoid fever has become almost stereotyped, one text after the other repeats the description with almost exact similarity, whereas considerable variation occurs in different cases.

(1) So few cases come under observation in the initial stage that there cannot be a certainty as to the exact manner in which the temperature rises in the majority. All the authorities say it is by a gradual step-ladder-like process, without giving due prominence to the, at least, not infrequent exceptions to this rule. Occasionally cases are met with in which the outbreak is marked by decided chill, followed by a rapid rise of temperature; in others—more numerous in my experience—the symptoms begin with vomiting and purging, like those of a choleraic diarrhœa, and the temperature is found high, or rises rapidly to its maximum. In both these cases these acute symptoms usually disappear in a day or two, and the ordinary course of typhoid fever follows. There is a possibility that such cases may have been developing for some days before, without producing sufficient discomfort to cause suspicion of the true character of the case.

In some, again, the rise of temperature is by an irregular zig-zag, reaching the maximum in five or six days; in others the rise is rapid, without unusual symptoms, the maximum being reached in two or three days; in these latter the duration of the fever is short and usually terminates by a pseudo-crisis. These atypical features are met with more frequently, and in a more marked degree, in children, whose temperature equilibrium is so easily disturbed.

(2) In regard to the daily range of temperature, it is generally considered that two observations, one taken during the forenoon and the other any time during the afternoon, are sufficient to give a reliable guide as to the range of the temperature for the day; that the morning observation gives the lowest record and the evening the highest. That this is a mistake we can readily convince ourselves by making frequent observations, as, *e.g.*, hourly, or every two hours. We know that in health the temperature varies somewhat from hour to hour, and in the febrile condition, as we should naturally expect, the temperature equilibrium is more easily disturbed, so that the variations from hour to hour should be greater than in health, and they are even so, being more marked in children, but even in adults the change may be as much as 2°.

This section of a chart (Fig. 1) illustrates this well. It is from a case of enteric fever in a child, and the observations were made as nearly as possible every two hours, when the child was awake. The first four records were made at 2.30, 7, 9, and 11 a.m.; the next four at 1, 3.40, 6, and 9.30 p.m.; the next at 2.15, 7, 9.30, and 11.30 a.m.; and the last three at 1.30, 4, and 6.30 p.m.

The observations of one day may be made when there is a decided exacerbation of temperature, and next day during a remission, indicating a lower temperature than on the first da whereas, had the temperature been taken during an exacerbation the second day, it might have been found considerably higher than on the first day, and indicate no improvement in the patient. We have all frequently met with illustrations of the truth of this; to-day the patient seems better, but the chart reports him worse, or it may indicate an improvement that we cannot find grounds for accepting. To ensure a correct temperature record it is necessary, therefore, that several observations be made daily; it is desirable in all serious cases that such should be done in order to protect ourselves against a misleading record.

(3) As to the duration of the febrile movement, there are many departures from the ordinary, which is considered to be from twenty-one to twenty-eight days. In the first place, in some—in my own opinion, in many—the fever aborts after a duration of from seven to ten days, or even less. On a former occasion* I described some such undoubted cases. The temperature elevation in such cases is usually moderate, but may be high; it is rapidly attained, usually in two or three days, and terminates, as a rule, by a pseudo-crisis. We meet with cases that are clearly abortive in households with one or more well-developed ones. There seems to be no more ground for denying the occurrence of such cases than of abortive attacks of any of the acute eruptive fevers. The changes in the bowel in such cases would probably not extend beyond congestion of the Peyerian patches in the lower part of the ileum, with, in some, slight ulcerations of the patches on, and in close proximity to, the ileo-cæcal valve. The opportunity for examination in such cases is so rare that there can be no certainty as to the bowel changes.

There is another class in which the elevation of temperature is of short duration. In these the temperature, never high, falls to normal, or even below, after a few days, but the disease—the fever—does not abort; on the contrary, the other phenomena pursue the usual course and convalescence does not begin until the customary time. The following case, under my care last September, is one in point: Dr. McG., a house physician in Toronto General Hospital, had the usual prodromal headache, malaise, and anorexia; then followed elevation of temperature for not more than a week, after which it was normal, or a little below it. The facies, the peculiar odor, and the coated tongue of typhoid fever, were present; the bowels were constipated; the prostration was quite as marked as it is after a typical course of average severity, and the convalescence was quite as protracted.

In a third class, instead of febrile movement, the temperature is normal or subnormal throughout. Strubé noted fourteen such cases in a typhoid epidemic among the German troops at the siege of Paris.* The usual symptoms were present, including delirium, which was furious in some. One died of complication, and the autopsy showed the typical enteric lesions. A few such cases occurred in the Toronto General Hospital some years ago.

I might here venture the suggestion that the febrile distendance of the third and fourth weeks of enteric fever is partly if not wholly due to septic poisoning. By this time the temperature becomes increasingly remittent, with more or less free perspirations, as occurs in mild pyæmia. This change in the temperature occurs when the sloughs are being cast off, and large ulcers, with decomposing tags of mucous membrane still adhering, have formed in the bowel—conditions favorable for the absorption of septic poisons. Cases terminating about the fifteenth day do not exhibit this remittent temperature, and the intestinal changes are seldom characterized by much, if any, sloughing.

II. *Relapses.*—Of more anxious concern to us, however, are the cases with protracted fever, either from complications or the occurrence of relapse. Our views as to the frequency of occurrence of relapses will vary according to our opinion as to what constitutes a relapse. Murchison defined it as "a second evolution of the specific febrile process after convalescence from the first attack is fairly established."† Some follow Irvine in describing two varieties of relapses, the intercurrent and the consecutive. By an intercurrent relapse is meant a re- or auto-infection at any time during the course of the primary attack; the consecutive corresponds to Murchison's. An intercurrent relapse may occur at any time during the primary attack; if it

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*CANADIAN PRACTITIONER, 1889.

*Quoted by Shattuck, *Boston Medical and Surgical Journal*, 1889, ii p. 221. † Murchison: *The Continued Fevers.*

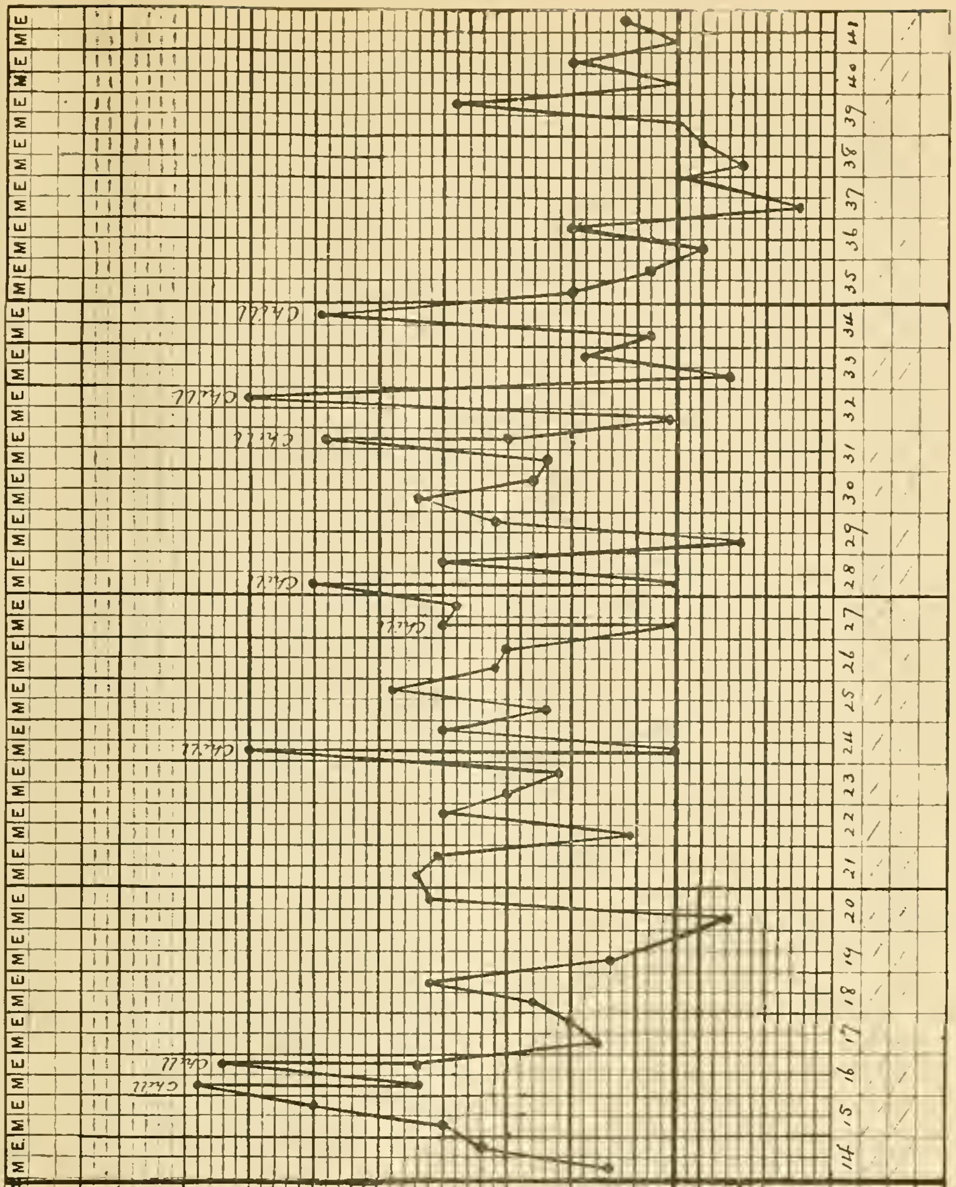

FIG. 2.
A CASE OF PHLEBITIS.

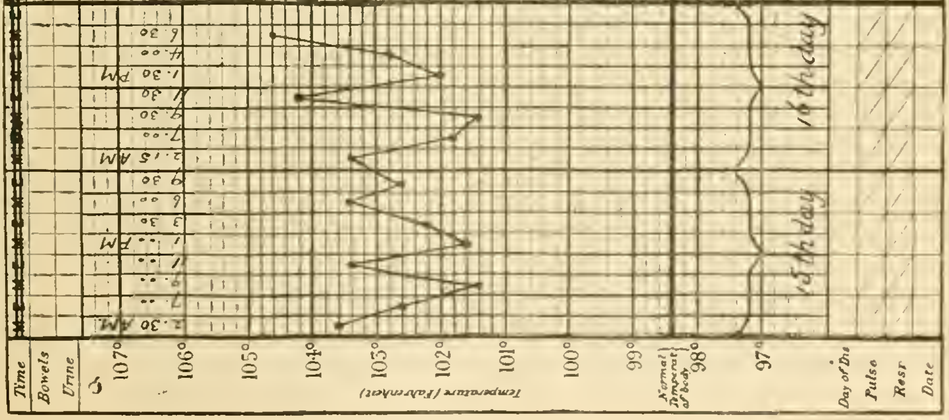

FIG. 1.
SHOWING VARIATIONS OF TEMPERATURE DURING THE DAY.

occurs early it may be impossible to differentiate it from the primary, but if it does not occur until towards the end of the third week, during the defervescence of the primary fever, and extend into the fourth or fifth week, or even the sixth, there would be no difficulty in distinguishing it, unless masked by some complication. According to this view, all cases in which the febrile disturbance is protracted beyond the fourth week at latest, possibly beyond the third, in absence of complication, are probably instances of intercurrent relapse. This is based on the theory that typhoid fever is a self-limited disease, lasting a definite time, beyond which it is never protracted, in absence of relapse. A consecutive relapse may occur at any time after defervescence of the primary fever, *i.e.*, after the evening temperature reaches normal, but it is usually not until five to ten days after that time that it commences; this has been so in five of my cases during the present year. As to the cause of relapse we are wholly in the dark, and we are therefore quite powerless in preventing them. Error in diet and too much movement, usually assigned as causes, may produce a sudden temporary rise of temperature, but true relapses nearly always occur quite apart from them. All the cases I have seen, except one, have arisen while the strictest diet and quiet were still observed. There evidently must be a re- or auto-infection, from what source we know not, possibly from the intestinal canal or from the spleen. The duration of a relapse is usually shorter than that of a primary attack. In twenty cases reported by Shattuck, the average was nineteen days.* Probably in many instances the attack aborts. The temperature attains its maximum usually in three or four days, more quickly than in the primary disease. A second and even a third or fourth relapse may occur. The prognosis is usually favorable.

III. *Phlebitis.*—Another phenomenon that occurs, sometimes as a complication, more frequently as a sequel to typhoid fever, is venous thrombosis, or phlebitis. The frequency with which it occurs varies in different seasons, and, apparently, in different localities, as many, having extensive experience, have never met with it. Chew, of Baltimore, has never had a case; he has seen four in consultation, which he describes in a recent article.* The works of Bristowe, Reynolds' System, Sir Thomas Watson, Trousseau, Loomis, and others, make no reference to it, and only a passing reference is made in that storehouse of clinical observation, *Fagge's Principles and Practice of Medicine.* I have met with phlebitis only during the past few years, and my colleagues say they saw no cases in the earlier years of their practice. It was described by Christism as occurring in Edinburgh from 1817 to 1820, and was noted in the London Fever Hospital a few years later. Both Murchison and Liebermeister describe several cases. It is therefore not a phenomenon of recent times only. Shattuck† met with it in nine out of 129 cases, and in my own experience it has been fully as frequent, while Macdonnell, of Montreal,‡ met with only two in 100 cases seen during the past few years.

The condition, so far as noted, is reported as occurring nearly always in the left femoral vein; no reason is suggested for this peculiarity. Of six cases of which I have notes, four were in the left femoral vein, or its branches in the calf; one in both, being much more severe in the right—this case I saw with Dr. I. H. Cameron in the General Hospital; and one in which both arms were affected simultaneously during a relapse.

In the femoral the phenomenon usually begins with sharp pain in the calf, or behind, sometimes above, the knee, with tenderness; this is followed by swelling, resembling phlegmasia alba dolens, with which the disease is probably identical. The vein, if superficial, can be felt as a hard, tender cord. Remittent febrile disturbance, sometimes with marked chills and rigors, is usually present, and may continue for several days, or even weeks.

This chart (Fig. 2) represents the temperature in a case under my care last winter. The chills were very severe; the fever disappeared a few days after the last chill, complete recovery following.

The chief danger is probably that a fragment of clot may become detached and cause fatal embolism. Troublesome swelling of the leg may persist for an indefinite time after convalescence, even for years.

In the case in which the arms were affected there was very little swelling, but they were extremely painful and tender along the inner side. The absence of swelling was due probably to the greater vascularity of the arms, the blood being returned by other and unobstructed channels. That the symptoms were not due to neuritis was evident from the absence of wasting, paræsthesia, and paralysis, other than that due to the pain.

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*Ibid.

* *Medical News*, 1890, ii p. 354. † Ibid. ‡ *Medical News*, 1890, ii p. 225.

A rare and instructive case of phlebitis occurred a short time ago in the practice of Dr. Uzziel Ogden, of this city. The patient, a young lady, had a fairly severe attack of enteric fever, followed by a relapse, which lasted till the end of the sixth week; then, instead of the expected convalescence, the temperature began to show evening rises again, without any cause being apparent. Notwithstanding this recurrence of febrile disturbance, the facies and other phenomena of typhoid fever disappeared, appetite began to return, and it became evident to so acute an observer as Dr. Ogden that, whatever the cause of the recurrence of fever, it was not a relapse. To improve her strength as much as possible, therefore, solid food was ordered, tentatively at first, but soon as freely as she could partake of it. She improved, although the temperature continued to rise, irregularly, in the afternoon, at times to 102° or 103°. After three weeks the cause of trouble showed itself in thrombosis, extending down the left femoral vein. Before this there was slight tenderness to deep pressure in the pelvis, for which there was no satisfactory explanation. The cause of this was now quite evident, being due, doubtless, to inflammation in the pelvic veins. Complete recovery took place in due time. It is quite probable that not a few protracted cases, for which no cause is apparent, may be due to phlebitis in veins beyond the reach of exploration.

The period at which phlebitis occurs varies; in some it occurs early during the fever, but usually not until convalescence has set in. Dr. H. T. Machell, of this city, related to me the cases of two sisters; in the first there was phlebitis of the left femoral as a sequel to the fever, while in the second it occurred early as a complication; in neither was the prostration marked.

The cause of phlebitis usually assigned is weakness of the heart, resulting in a languid circulation, with probably some change in the condition of the blood. That this is not the correct explanation is quite evident from the fact that it is not the very prostrate that are usually affected, and that it often develops before the fever has lasted long enough to cause prostration. Most authorities look upon the condition as a thrombosis with secondary phlebitis, but the early occurrence of pain, tenderness, and fever, seems to negative that theory. That it is a phlebitis, or an endo-phlebitis, with thrombosis resulting, there can be little doubt; and that the immediate cause is septic is rendered almost certain by the pyæmic character of the consequent febrile disturbance (*vide* Chart II.) What the nature of the sepsis is, and whether it is the same in all cases, requires further investigation to decide. The probabilities seem to be that the phlebitis occurring during the fever is directly due to bacteria of typhoid, which may, almost certainly, under varying circumstances, produce a variety of pathological conditions. Phlebitis occurring as a sequel is probably caused by the absorption of septic poisons from the intestines, where they are generated by the processes of ulceration and decomposition.

The frequent occurrence of pneumonia as a complication of typhoid fever seems explicable on no other theory than that the inflammation in the lung is directly due to the typhoid germs. The occasional occurrence of abscesses, single or multiple, in this disease, may be due to the same cause, or more often to the septic matter absorbed from intestinal tract.

In a case under my care two years ago, towards the end of the illness several small superficial abscesses formed. Single abscesses are more common. Occasionally a joint suppurates, or multiple pyæmic abscesses may form in the lungs and elsewhere. Suppuration of the middle ear, in some cases double, occurred in four or five cases in the Toronto General Hospital, this autumn, and may possibly be explained in the same way, though this and parotitis are usually attributed to extension of inflammation from the pharynx along the eustachian tube in the former, and from the mouth along Steno's duct in the latter.

The occurrence of such various complications points to a great variety of poisons causing them; and probably, phlebitis may be dependent on poisons equally various.

Fortunately, cases complicated with phlebitis rarely prove fatal; the clot usually undergoes red softening apparently, and is absorbed; the wall of the vein becomes normal and its potency is restored. Possibly in those cases of persistent œdema the vein becomes permanently occluded by adhesive inflammation. Occasionally the clot becomes puriform and a fatal termination results from sepsis.

IV. *Post-Febrile Insanity.*—In common with other acute diseases, typhoid fever is occasionally followed by some form of mental disturbance which may show itself as active mania, melancholia, hallucinations, or loss of mental power. No sequel could be more distressing than this. Similar disturbances are met with occasionally by surgeons after operations,* and in obstetric practice it occurs as puerperal insanity. Dr. H. C. Wood† has attributed the cause, in all, to exhaustion of the nerve centres, resulting from defective nutrition. A striking case came under my care last January, in the person of a young woman who had entered the General Hospital in November with typhoid fever. There was considerable delirium; the febrile disturbance was continued by abscess of the left parotid gland; the delirium gradually increased and became noisy, with the development of delusions. She came under my care on the first of January, and by the middle of that month the parotid abscess had recovered, but the alienation persisted and became more delusional. She became so unmanageable that removal to the Asylum in February became necessary. Of her history there, Dr. H. E. Buchan, assistant medical superintendent, kindly furnished me the following account: "She entered February 10th, 1890; she was noisy, restless, excitable, and violent at times, claiming in turn to be Mary Queen of Scots, the Saviour, and others; in fact, hers was a very typical case of acute or active mania. There was marked physical debility." Under good nourishment and tonic remedies she gradually improved, was sent home in May on a month's probation, and discharged as "recovered" on June 6th last.

The one cheering feature of these cases is their hopeful prognosis. So far as I am at present aware, they all recover within a few months. Osler‡ reports one of five cases still subject to hallucinations, but improving, after four months. In view of this fact it is very desirable that, when possible, all cases should be treated at home, to avoid the unpleasant after-feeling that attaches to incarceration in an asylum. The treatment would consist in careful, constant watching, the fullest possible nourishment, tonics, good hygienic surroundings, massage, and rest in bed. The asylum would be far preferable, however, to home treatment, if the fullest care could not be bestowed. With restored physical health, the mental condition will usually regain its equilibrium also.

Dr. R. S. Tyrrell, of this city, has kindly communicated to me the particulars of two cases of melancholia, with mental weakness bordering on imbecility, in young men. One had typhoid in August and the other in September; both had relapses of short duration, the mental weakness following. One is still debilitated, but the other has regained his physical strength but without improvement in his mental condition. Neither of them have had any delusions.

V. *Pylephlebitis.*—This is a rare and untoward result of typhoid fever. One case occurred in my hospital service last year, in a man who had passed through a moderately severe attack of fever. As he began to convalesce the temperature became irregular, the changes becoming more marked as time passed. Chills occurred from time to time, with sweating, and jaundice gradually developed, with some enlargement of the area of liver dullness. Later, the abdomen gradually enlarged and the signs of chronic purulent peritonitis developed, masking to a certain extent the signs of the hepatic fever. He died about four weeks after the recurrence of the febrile disturbance. The autopsy revealed chronic purulent peritonitis, with considerable plastic exudate and a diffuse pylephlebitis, the portal vein throughout all its ramifications being filled with puro-lymph. In the mesentery, near the ileo-cæcal junction, was found an abscess, 1½ inches in diameter, originating, doubtless, in a mesenteric gland, and causing both, first, the pylephlebitis, and, later, the peritonitis. The diagnosis in such cases can seldom be more than probable; and when made, it is seldom that anything can be done to give relief.

VI. *Constipation.*—Among the first symptoms of typhoid fever named by almost any medical student is diarrhœa; it is one of the first symptoms given in all our text-books; yet reference to late reports of this disease shows that it is

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Shepherd, in *American Journal Med. Science*, 1888. Thomas, in *Med. News*, 1888. †*University Medical Magazine*, 1889. ‡*Johns Hopkins Hospital Reports*, Vol. II., No. 1.

present prominently in only a minority of cases. Thus, in Shattuck's 129 cases referred to it was absent in 51 per cent. (66 cases), and it was prominent in only 25 per cent. (32 cases). In Macdonnell's 100 cases it was present in only 27. In the Toronto General Hospital, during the last three months, the results, so far as I can ascertain, accord very closely with these. Of course, the frequency of diarrhœa varies somewhat in different seasons. Comparing these statements with Murchison's, the difference is very remarkable. He found diarrhœa present in 93 per cent. of his cases, and his contemporaries give even higher percentages. What is this change due to? It can scarcely be owing to a difference in treatment. It is extremely desirable that fuller statistics should be obtained on this point and its bearing on the prognosis and treatment. No doubt the fatality in the diarrhœa cases is much higher than in those with constipation.

Addison first, I think, and, later, Sir William Jenner, suggested that in all diarrhœa cases the colon is more or less extensively ulcerated. In all the autopsies, not many of course, that I have seen during the last three years, the colon has been found ulcerated in the diarrhœa cases, and unaffected in the constipated ones. More extended observations are desirable on this point.

While constipation is generally of favorable prognostic significance, yet in some of these cases the signs of grave ptomaine poisoning are severe in the extreme. It would seem as if the excreta, being retained in the intestinal canal, the poisons are absorbed freely into the system. I had two such cases two years ago, a man and woman. In the woman, especially, prostration with hebetude was marked from a very early period; there was complete loss of consciousness for two or three days before death. At the autopsies, extensive ulcerations were found in the ileum in both, but no disease of the colon. Broadbent[*] has recently referred to such a case, seen at the end of the second week, in which with the constipation there was, besides, extreme prostration, weak pulse, and cold, clammy extremities. The outlook was almost hopeless: the nervous system was obviously overwhelmed by some poison, doubtless the ptomaines absorbed from the intestinal tract, and the only hope was to have the bacteria and their products swept from the alimentary canal. With this object in view, three grains of calomel were given, the duration of the disease not having yet been sufficient to render such a course dangerous, except, perhaps, from hemorrhage. Next day, after the action of the calomel, there was much improvement; another dose was given and the case resolved itself into a mild case of enteric fever, and ended in recovery. Such a report as this, from a man so capable and so worthy of confidence is not only interesting but extremely valuable to us, from a practical point of view, in giving us the necessary courage to act with judicious boldness when similar occasions arise.

VII. *Peripheral Neuritis.*—This complication is of much more frequent occurrence than I supposed before examining the literature. The symptoms of it were fully described by Trousseau, but attributed to disease of the spinal cord: he says, however, that no lesion of the cord could be found in any of the autopsies.[*] Shattuck reports three in his 129 cases of typhoid,[†] and Folsom, eight in 183 cases.[‡] I have not met with any cases myself. My colleague, Dr. J. E. Graham, has furnished me with the notes of a case under his care in the General Hospital in January, 1887. She had fever in September preceding; both legs became powerless during convalescence: she also complained of loss of memory; there were fairly well-marked evidences of peripheral neuritis, involving both lower extremities. The ultimate result is not known, as she has been lost sight of.

VIII. *Meningitis*, like pneumonia, may probably be caused directly by the typhoid germ. The symptoms so mask those of the fever that a diagnosis is often impossible. I am indebted to Dr. Graham for the notes of a case of which the following synopsis gives the salient points. A young bank clerk complained of indisposition, with great drowsiness, often falling asleep over his books; no headache; the drowsiness increased till soon he was constantly sleeping, with intervals of delirium. Temperature only slightly elevated and irregular. The coma deepened, with a few conscious intervals, until death. *Autopsy* showed some inflammatory effusion into the pia mater, with deep congestion of all the intra-cranial vessels. In the intestine were found the characteristic lesions of typhoid fever. There remains the possibility that the intestinal lesions were the result of an indifferent irritant, if Harley is correct in his views that such can occur.[§]

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[*] *British Medical Journal*, 1890, ii., 781.

[*] Clinical Medicine.

[†] Ibid.

[‡] Ibid.

[§] *Reynolds' System of Medicine.* Article: "Enteric or Typhoid Fever.

TYPHOID FEVER.

BY WILLIAM OLDRIGHT, M.A., M.D.,

PROFESSOR OF HYGIENE IN THE UNIVERSITY OF TORONTO.

A Paper read at the Post-Graduate Course of the University of Toronto, Dec. 19, 1890.

IN this paper I shall say very little about the type of the disease during the recent epidemic, and about treatment, as these will, no doubt, be subjects to which my colleagues of the department of Practice of Medicine will direct your attention at greater length.

I fear that in connection with the epidemic which has prevailed throughout the province, and in various other portions of this continent, Toronto has received more than its own share of prominence. It is not my intention to point the *tu quoque* finger at any other individual locality by name, but I have gathered from provincial and other authorities sufficient data, and I may say in general terms that there are at least two cities in the province whose ratio of cases to population are greater than that of Toronto; one village in which it has been double, and one township in which it has been 50 per cent. greater.

My available figures from cities south of the border are limited to two; they show in one large city a slight increase over us, as shown by the death rate; in the other the ratio of cases this year is only half of ours, a decided decrease having taken place there as compared with last year; still we should not continue in our sanitary transgressions even in company. Toronto has been noted as a healthy summer resort for our neighbors to the south, and should seek to regain and maintain her reputation in that respect.

Laboratory and other observations on the life of bacilli of typhoid.—It is not my intention to refer to all the facts which have been made known in regard to the life, history, and reproduction of the bacillus of typhoid, but only to a few which, it has occurred to me, may throw practical light upon the causation, spread, and restriction of the disease.

The growth and reproduction of typhoid bacilli outside the body has been demonstrated in the laboratory. It has been shown that for their successful cultivation a certain amount of organic matter is necessary. Dr. Meade Boulton has found that in beef broth, containing 6.7 parts per 100,000, they will live for months. The cultures can also be more successfully manipulated with certain forms of organic matter than with others. Conversely, it has been shown that the absence or great scarcity of organic matter is unfavorable to their development, that in a class of waters designated "potable waters" they will die out in a brief period—say, six days.

Interesting observations have been made in regard to the action of various kinds of micro-organisms (and amongst others the typhoid bacillus) on each other. Temperature has been found to have a decided modifying influence. Vaughan found that at a temperature of 20° C., or 68° F., the typhoid bacilli would increase in such numbers as apparently to kill out the ordinary water forms. It has been observed that at lower temperatures their vitality is impaired, and that at 46° F. they would disappear in six days, the ordinary water forms maintaining their supremacy. Prof. Dixon, of Philadelphia, obtained similar results in five days with typhoid bacilli in Schuylkill water.

At this point, however, we must guard against an incorrect argument that might be based on the fact just mentioned; I refer to the fallacious supposition that impure water will yield pure ice, and that freezing will kill pathogenic germs. The endemic at Plymouth, Pa., four or five years ago, has furnished us with one of the notable proofs of the fallacy of this supposition. Plymouth is supplied from four reservoirs at different elevations, fed by a mountain stream. When the supply to these becomes scarce, water is pumped from the river. This was the case from the 20th to the 26th March, the lower three of the reservoirs being nearly empty; but on the 25th a thaw set in, and on the 26th the upper two reservoirs became full; the water was let down from the third into the fourth, and the town was supplied from this water. About fifteen days later an epidemic of typhoid broke out. It was afterwards ascertained that during February, and portions of January and March, the dejecta of a typhoid patient had been continuously thrown on the snow and frozen ground, within seventy feet of the bed of the stream, a short distance above the third reservoir. The thaw which set free the mountain stream and filled the reservoir also carried into the lower two the frozen accumulations from the typhoid patient.

The direct bearing of such observations on our ice supplies is obvious, and I must here commend the action of our health authorities, last winter, in stopping the cutting of ice from the bay, and the fairly ready acquiescence of ice dealers and their enterprise in obtaining good supplies from purer sources in the less densely populated portions of the province.

Whilst a low temperature so lessens their vitality that they gradually die out, freezing suddenly stops all vital action and keeps them stored for action in the future. Another way in which the bacillus is stored and propagated is by being dried and pulverized, as when excretal slops are thrown upon the ground, dried, and blown through the atmosphere. The experiments of Kraus show that the vitality of the typhoid germ is greater than that of cholera—that it will live longer in the struggle with the ordinary water forms.

Last year Karlinski published in the *Centralblatt* the results of investigations regarding the behavior of typhoid bacilli in typhoid dejections. He arranges them as answers to the following questions:

1. In what time can the specific typhoid bacilli be demonstrated in the dejections of patients?
2. Can the demonstration of typhoid bacilli in the fæces be looked upon as a diagnostic sign?
3. How long does the vitality of bacilli in typhoid dejections last?
4. How long do the bacilli in typhoid stools, in pits, retain their vitality?

Search was made from the fourth day onwards, dating from the initial chill. The bacillus was not found before the ninth day in any case. In the greatest number of cases (nine) it was found on the fourteenth, the range being very wide—from the ninth to the twenty-first; the actual numbers being two on the ninth, two on the tenth, four on the twelfth, nine on the fourteenth, two on the seventeenth, and three on the twenty-first. The time at which they began to disappear seemed to have a marked coincidence with the fall of temperature in the patient and the cessation of diarrhœal stools. In many cases this was about the twenty-fourth day, and in no case were they found later than the fiftieth day.

The bacilli in the stools did not in any case retain their vitality for more than three months. This limitation, if substantiated by later observations, is worthy of note; showing that in its natural medium the bacillus dies out more rapidly than if it starts off and finds fresh fields and pastures new under favorable conditions. The keeping of them in different temperatures had no noteworthy influence on the duration of the vitality in the stools; but the addition of some other forms of bacteria had.

Modes by which the disease is spread.—I need not dwell upon the numerous methods by which the typhoid bacilli obtain admixture with our food and water, and gain entrance into the *primæ viæ*, nor upon their immense multiplication in the different media. I would, however, like to draw your attention to a "Report on an endemic of typhoid fever at Springwater, New York," published this year. Springwater village is situated in a valley of the same name, two and a half miles south of Hemlock lake, a lake about six and a half miles long, from which the reservoirs supplying Rochester are fed. Springwater village is situated on the main affluence of Hemlock lake. By a calculation given in the report, it seems that under certain conditions germs might travel from Springwater to Rochester in thirty-six hours. It is this danger to their water-supply that led the authorities of Rochester to institute an investigation. The first case observed in Springwater was that of Orson Grover, a boy who was supposed to have imbibed the bacilli from a well in close proximity (30 ft.) to the privy of the village inn, and fifteen feet away from a slop drain. It is supposed that the privy had been infected by some case of ambulatory typhoid. The boy Grover was removed to his mother's house, and a second pit was thus infected, which by soakage infected the schoolhouse well, and by similar repetitions and interchange the epidemic rapidly spread.

One question is sometimes raised: Can the germ be carried through the air by ascensional currents? I think there are few of us who have not met with cases where the circumstances have been such as to confine us for an explanation to the action of sewer gas obtaining entrance into the houses where they occurred. One of the most striking examples of this occurred to me in a family where five members were threatened with low fever. The plumbing being found fairly satisfactory, I directed an examination of the weeping drains to be made, and when one of them was opened, a strong and continuous current blew up into the cellar. The drain had forgotten to weep, and its trap had become dry and useless. Two members of the household became ill with typhoid; one of them so ill that Dr. J. H. Richardson was called in in consultation. I mention this in corroboration of the diagnosis. The condition of the weeping drain was at once rectified. The other three members escaped without a well-marked attack.

There are no authenticated instances, so far as I am aware, of infection with the disease from the ascent of typhoid germs from bodies of still or comparatively still water in the open air, but it will readily be seen that it would be difficult to dissociate this condition from other circumstances.

With water splashed through the air in the shape of spray or foam, the case is different. This, and the fact that bacilli may be wafted in the form of dust, should be borne in mind before filling watering-carts from a filth-polluted bay! In connection with the epidemic through which we have been passing, one interesting fact has been brought to my attention by a professional friend and colleague. A slight secondary outbreak which occurred in a public institution at a time when the epidemic had pretty well subsided elsewhere, he attributes to the fact that drinking water is drawn from a cistern in the building. Some of the germs, having been introduced into the tank at an earlier period, have found in the organic silt and increased temperature a suitable culture field. The danger of domestic tanks has often been pointed out, but we have not had illustrations so near home. And in this connection it would be as well to remind our fellow-citizens that many of our baths and closets are supplied from tanks, and that from the taps of the former many persons will take an occasional drink. It would therefore be well that at the present juncture *pater familias* should have his tank thoroughly cleaned out, a duty to which he should at all times attend at regular and frequent intervals.

The general conclusions to be derived from our experience, and strengthened by the foregoing observations, are:

That we should have no dead organic matter lying idly around as fodder for the germs of disease.

That all such matter should be destroyed or by some safe disposition returned to living organisms or to innocuous decomposition.

That we should deal actively with the very first case or cases of infectious disease, and destroy the germs; that they can be more readily dealt with by active treatment in the first case than when they have multiplied.

These abstract general principles will perhaps appear less formidable if we apply them to specific details and illustrate them by reference to specific cases. Taking them in order, let us apply the first to

Our water supplies.—The scheme of obtaining water from Lake Simcoe has been resurrected and is again being discussed. The very first question that ought to be decided is whether Lake Simcoe water is the best for us, even were it at our door. In a published report by Prof. Ellis, different samples of it are rated as second and third class, that of Lake Ontario being first-class, the water of Lake Simcoe containing a greater quantity of organic matter; and it has been shown that organic matter is favorable to the growth and multiplication of typhoid bacilli. Again, will it be any more feasible for us to safeguard the waters of Lake Simcoe and the intake and channel leading from it than to protect the much purer water in our own neighborhood? This last remark leads to a second question that is awaiting solution. It was recommended by Prof. Galbraith four years ago, in a report in which I was associated with him, that a sufficient number and variety of float experiments be made to determine where our sewage could be emptied without danger of contaminating our water supply. These two points should be positively determined before any further engineering schemes are elaborated, and the determination of them could be made at a cost trifling in comparison with the outlays that have been made in connection with engineering schemes to which they should have been preliminary. In country districts the cleansing of wells and their protection from soil pollution and surface pollution will come under this head, and where rain water is used, the frequent cleansing of tanks, roofs and eave-troughs.

The means of keeping the soil free from accumulations of organic matter are obvious. I cannot, within the limits of this paper, enter into the details embraced under the following heads :

Abolition of privy-pits and cess-pools.

Substitution of systems of dry-disposal and sewerage.

Cremation of refuse.

These reforms have been urged again and again by our late medical health officer, and the subject of crematories is even now being pressed by our acting medical health officer, the Chairman of the Board of Works, and others. It has been agitated in the past again and again by our City Commissioner, but still we have no crematory, and indecision as to the experimental expenditure of a few thousand dollars has, with other neglects, cost the city a hundredfold the amount required. And in the refuse dumps throughout the city we have stored up choice building sites for the future. In this connection it may be of interest to show diagrams of some of the crematories which are being discussed, and which some persons here present may not have seen.

The freeing of air from organic impurity by means of sufficient ventilation, and doing away with over-crowding (as well as by avoidance of soil pollution just referred to), is a subject which would require a paper of its own.

I think that practically there is not sufficient care taken to destroy the germs in typhoid stools. The best that is done, in cases where the subject receives attention, is to put a solution of 1 in 500 of bichloride of mercury in a vessel to receive the dejecta, cover them over with the same, and at once empty their contents into a closet, privy-pit, or hole in the ground. How can this be expected to act on the bacilli? If this plan of destruction is aimed at, the bichloride solution and dejecta should be stirred together, and the mixture allowed to stand long enough to give the bichloride an opportunity of acting. The fresh fæces will not endanger the operator. The piece of stick used can be kept in bichloride solution. Where there are furnaces with capacity for good fires, cremation should be adopted. In this case the dejecta can be received into vessels containing carbolized saw-dust. This will protect the walls of the vessel and facilitate the cremation. These (metallic) vessels can be provided with close covers.

If it be remembered that soiled linen is not placed in the first instance in boiling water by the washer-woman, but receives a preliminary washing in merely warm water, which may be afterwards thrown out in the yard or poured down a sink, it will be apparent that the precaution of soaking such linen for some hours in a germicidal solution is absolutely necessary. Some are too apt to rely upon the boiling alone. A new and striking illustration of this is given by J. C. Wilson, of Philadelphia. A serious endemic existed for years among the German soldiers in a certain garrison, from linen not properly disinfected, and ceased when this precaution was properly attended to.

In these matters the co-operation of the health officer and his staff ought to be courted rather than resented : and my opinion is that for systematic disinfection, personal oversight by a *competent* inspector is at first, and from time to time, necessary.

I would here emphasize the necessity of prompt reporting of infectious diseases, for how otherwise can the germs be stamped out in the incipiency of what must otherwise become an epidemic?

Local boards of health. I would also add my voice to the representations that are from time to time made regarding the composition of these boards. It does not need much argu ment to convince the ordinary individual that men who understand disease, sanitary architecture, plumbing, engineering, chemistry, and biology, are more fitted to deal with sanitary matters than men who have no knowledge of these subjects.

Practical scientific work.—Having referred in this lecture to the progress, side by side, that is being made in scientific, biological and chemical investigations, and their practical application to the prevention and cure of disease, I cannot refrain from alluding to the work that is being carried on by this University and in this building. Can it be shown that the scientific labors of Professors Ramsay Wright, Ellis, Pike, Loudon, and Macallum, have been retarded by association with their colleagues in this Medical Faculty? Do they not rather derive additional stimulus and satisfaction by the practical application of a portion of their work to various problems of surgery, medicine, and hygiene? I need not ask you if this association and collaboration has diminished the interest of either the profession or the public in University work, for your presence at these lectures and the attention with which you have listened to them render the question unnecessary; and I know that in regard to this faculty of the University, as well as in regard to others, you will join in the expression of our university motto: "*Velut arbor ævo crescat, velut lampas luceat.*"

That our Vice-Chancellor recognizes the value to the people of such association of scientific research and its practical application is evinced by his recent generous conduct, and the letter in which his proposition was made known to the faculty.

Nor would I close a paper in which I have referred so often to bacteriological investigations without expressing the gratification of many of us at another new departure which has recently been made. I refer to the inauguration of the bacteriological laboratory in connection with the Provincial Board of Health, in which at the present time Mr. J. J. McKenzie, B.A., a graduate and recent fellow in biology of this University, is carrying on researches in connection with the typhoid bacillus. I am sure you will join with me in expressing appreciation of the wisdom of the Government in aiding this onward movement for the preservation of the public health, recognizing once more the motto of the Provincial Board of Health:

"*Ne pereat populus scientia absente.*"

TUBERCULAR DISEASE OF BONE---PATHOLOGY AND TREATMENT.

By A. PRIMROSE, M.B., C.M. EDIN., M.R.C.S. ENG.,

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A Lecture delivered at the Post-Graduate Course of the University of Toronto, Dec. 18, 1890.

Mr. President and Gentlemen:

There is no field of surgery at the present time in which greater diversity of opinion exists than in the treatment of tubercular joint disease. Extreme views are held, on the one hand, by surgeons who advise an early attack on the diseased articulation, and complete removal by exsection of the joint; on the other hand, another set of surgeons believe that rest for the joint, combined with a strict regime of constitutional treatment, will prove more satisfactory even in advanced disease. There exists yet another class of surgeons who advocate a middle course, and very recently the suggestion has been thrown out that simple incision into the joint may result favorably, much in the same way as an incision into the peritoneal cavity in tubercular peritonitis exercises a curative effect.* The present methods of treatment of tubercular disease in the shaft of a long bone are scarcely more satisfactory than are those adopted in joint diseases. The disease runs a tedious course, and after many months or years may end in extensive necrosis of the bone, and eventually the limb may have to be amputated. In more favorable cases the disease may have been checked and a cavity may be left in the interior of the bone, the walls of which are healthy. These cavities may close and the case terminate successfully, but very frequently the granulations lining the walls of such a cavity become unhealthy, and the healing process may be indefinitely postponed. A case came under my observation in which such a cavity of large size existed in the upper end of the tibia in a boy, which refused to heal. An attempt was made by introducing desilicated sponge, after the method of Hamilton,* to encourage the growth and deposition of healthy tissue in the part, but this failed, as did every other device. Eventually the limb had to be sacrificed and amputation was performed at the knee joint. Certain cases, however, progress to a successful termination; the disease is removed, the tissues take on a healthy action, and the limb is restored to a normal condition.

The development of tubercle in osseous tissue, in the early stages of the disease, has been studied more minutely in the articular ends of the bone rather than in the shaft. The reason for this is obvious; operative procedure has many times been resorted to very early in the treatment of tubercular joints, and the method of removing the end of the bone entire has rendered it possible to examine the condition of the tissue without disturbing the relation of the structures entering into its anatomy. On the other hand, it is rarely, if ever, the practice of the operator to exsect a portion of bone entire from the shaft; when disease exists there it is removed piecemeal, and the same opportunity is therefore not afforded for minute examination. The disease seems primarily to start in the cancellous tissue of the bone where small grey nodules may be found; these multiply and coalesce, forming irregular areas; the collection

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Lancet, November 15th, 1890, page 1018.

Vide Hamilton, *Edin. Med. Jour.*, November, 1881.

then tends to caseate. The cartilage over the articular surface of the affected area of bone may be normal in appearance. In a case of early hip-joint disease I assisted Mr. Watson Cheyne in the removal of the head and neck of the bone; the following appearances I quote from my notes of the case: "The articular cartilage seemed healthy, but the ligamentum teres was soft and of a somewhat gelatinous consistence. The appearance of the bone, on external examination, was normal; on making a longitudinal section through the neck and head it was found that the cartilage was separated from the osseous tissue below, on which it was freely movable; there was an inflammatory condition immediately beneath the cartilage; the bone in the immediate neighborhood was softened and carious; a small focus of yellow caseous material, the size of a threepenny piece, existed in the substance of the neck in the anterior and lower portions; another focus was present in the cut edge of the femoral neck; there had evidently been a portion of this focus left unremoved in the lower fragments of the femoral neck." These caseous foci may still further break down, and cavities may thus be formed; the tubercular network of osseous tissue being destroyed, the diseased area is surrounded by bone in a st te of irritation, the blood vessels are dilated, exudation of the leucocytes occurs, and a collection of these cells, plus broken down tissue, is found in the cavities thus formed; we have in this way established a collection of pus in the cancellous tissue. The stages then in the development of tubercle may be summed up thus: (1) The typical grey tubercle; (2) the caseous mass; (3) the formation of a cavity; (4) a collection of pus with the entire destruction of the osseous tissue.

If we enquire for one moment into the minute anatomy of these conditions, we will find that the tubercular process, as manifested in bone, presents the same characteristics as it does in other tissues, modified somewhat, however, by the peculiarities of the osseous tissue in which it grows. If the grey tubercle be examined under the microscope we find one or more giant cells; surrounding these is a zone of cells of irregular outline, and outside these again a zone of round cells like leucocytes. Between the cells is a network of fibrous filaments. These appearances are so familiar that I need only mention them. A necessary item, however, among the constituents of the tubercular nodule is the bacillus tuberculosis, which is, we are bound to consider, always present, at all events during the earlier stages of the development of the mass. The life history of the nodule is determined by the fact that it is extravascular; there are no blood vessels among the cells; the osseous tissue in the immediate neighborhood shows an increased vascularity and an increase in the number of cells; these blood vessels, however, do not enter the nodule. A study of these special characteristics enables us to understand the peculiar features presented by tubercle. The fact that it is non-vascular explains the fact that these nodules do not grow beyond a certain size; further, the nutrition being necessarily defective, the nodules tend to undergo retrogressive changes, and we have, in consequence, fatty degeneration, the formation of a necrotic mass, and finally a caseous nodule is produced. We may find tubercle in isolated patches or nodules such as I have described, or, what is more common, a number of these coalesce and form an irregular mass of cells, which will eventually undergo retrogressive changes. The tubercular nodule does not always caseate; occasionally we find a sclerosed condition at the seat of the disease; this condition of affairs* has been described as follows: In the centre we find an accumulation of cells such as I have described. Immediately beyond this there is a growth of osteoblasts, deeply colored; this color, it is suggested, may be due to the presence of micro-organisms. After a time the osteoblasts form a mass of sclerosed bone; in fact, we have a tubercular deposit to begin with, which engenders a condensing osteitis in a limited area of bone, and so the formation of a non-vascular solid lump is the result: this may be in the very centre of the cancellous tissue of one of the growing bones. This sclerosed mass acts as a foreign body, sets up irritation, and may be the source of an inflammatory and suppurative process in the surrounding bone.

Tubercular caries. By the term "caries" is meant a gradual destruction and liquefaction of the bone elements, the results of an inflammatory process. This may be brought about by more than one set of pathological changes, but that a particular variety of caries is due to tubercular osteitis is beyond a doubt. In certain cases of caries we find tubercle in the affected bone; we find also that caries of the articular end of a bone is often a secondary development in typical cases of white swelling of the joint; then again, the bacillus tuberculosis has been demonstrated in the discharges* from an abscess dependent upon caries of the bone. Bacilli can scarcely ever be thus demonstrated in the pus, but that their spores are there can be shown by inoculation on the rabbit's eye.† The effect of a tubercular osteitis is to cause the bone to soften, the medullary tissue disappears, the cancellous trabeculæ are gradually destroyed; the bone in this way becomes first friable, so that if a probe were introduced and gentle pressure made upon it, it would readily be made to penetrate the friable tissue, which gives way before it; eventually a cavity is formed; when complete destruction of the osseous tissue has taken place, this cavity is filled with earthy granular particles, broken down cells, blood corpuscles, oil globules, and pus cells. In tubercular caries, therefore, we have a molecular disintegration of osseous tissue, the result of a previous tubercular osteitis; in this way an abscess is formed which may be restricted to the interior of the bone, or it may burrow in the adjacent soft parts, forming what is known as a cold abscess; these abscesses may point on the surface and open spontaneously, leaving fistulous tracks along which the discharge finds its way. Unhealthy florid granulations spring up and line the walls of such sinuses, and an exuberant growth of this granulation tissue exists about the mouth of the sinus. If the abscess remains unopened, we have a process of chronic inflammation in the soft tissues which form the abscess walls; this causes a thickening and condensation of the connective tissue, the innermost layers of which are badly nourished, and finally perish and are cast off into the abscess cavity as slough and shreds of gelatinous tissue in a necrotic state. The inflammatory process then attacks more extensive areas of the soft tissues as the abscess increases in size, and the wall of the abscess towards the cavity continues to cast off sloughs in the manner indicated.

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* Macnamara: *Diseases of Bone and Joints*, p. 108.

The condition of caries as restricted to the bone itself may extend indefinitely, and cases occur in which the entire bone in this way ulcerates, but it is more usual for the bone in the immediate neighborhood to become the seat of a chronic inflammatory process which leads to condensation and thickening of the surrounding bone; thus a carious cavity may be bounded by walls of sclerosed bone.

Caries-Necrotica. The process of liquefaction may continue solely as such, or there may be along with it death of comparatively large areas of bone, thus forming sequestra. There are two ways in which portions of bone may thus perish. We have seen that occasionally accompanying the production of tubercle we have a sclerosed mass of bone immediately surrounding a caseous nodule; this is non-vascular, and remains as a necrotic nodule, exciting irritation in its immediate neighborhood as a foreign body; this is a sequestrum really of of new formation. More frequently we find thin lamellæ of bone lying loose in the carious cavity; these portions of necrotic bone are cast off in the same manner as sloughs are detached from the abscess wall in the soft parts. The area of caries is surrounded by bone in the condition of condensing osteitis. As the sclerotic process advances, the Haversian canals are obliterated during the chronic inflammatory process in the compact bone, and the vascular supply cut off, or the vessels suffer from direct pressure of inflammatory effusion in the cancellous tissue. This occurs more particularly in the deeper layers of the sclerosed wall, and as a result, sequestra are cast off and are found lying free in the carious cavity. The conditions which determine the separation of a portion of bone in this way are dependent upon an exacerbation in the inflammatory process surrounding the diseased area. If caries run a slow course, and, during the disease, the limb is not submitted to any sort of injury or unrest which might determine a certain degree of irritation in the affected part, then we shall probably have the disease running a course of caries pure and simple; if, however, the process be a rapid one, or if inflammation is increased in the parts by injury or manipulation, or possibly by some general constitutional disturbance, then follows an extra amount of inflammatory effusion, pressure on the vessels

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*Roberts' *Modern Surgery*, p. 314.

†Erichsen: *Science and Art of Surgery*, 9th ed., vol. 1, p. 1[illegible]

in a confined area, and, consequently, death of the tissue from defective blood supply.

Necrosis. This is more common in the compact bone. In this condition the tubercular osteitis spreads rapidly throughout a large area of bone, and before liquefaction can occur, *i.e.*, before sufficient time has elapsed for the complete disintegration of the osseous tissue, the portion of bone affected is killed *en masse*, and a large sequestrum is formed; this sequestrum is set free by absorption of the bone in the inflamed area. The osseous tissue in the line of separation exhibits appearances similar to that found in caries; the inflammation here is of a specific nature and is in reality a condition of tubercular osteitis. Beyond this, again, the living bone is undergoing a process of condensing osteitis, so that there is great thickening of the shaft of the bone. Thus we have in tubercular necrosis to deal with three zones of diseased tissue, (1) the sequestrum, (2) the area of rarefying tubercular osteitis, (3) the areas of condensing osteitis. (The specimen exhibited demonstrates the condition described.)

I have now completed the description of the pathological phenomena observable in conditions of tubercular affections of bone, and you will observe that three distinct clinical varieties present themselves. Of these, caries is more common in cancellous tissue, in the articular extremities of long bone, and in the bones of the carpus and tarsus. It is also familiar in caries of the vertebræ, constituting what is known as Potts' disease. We may, however, find caries affecting any of the bones of the skeleton. Caries necrotica is met with chiefly in the shaft of long bones, and tubercular necrosis in the same situation. The condition described by the older writers as spina ventosa is primarily a tubercular osteitis, causing expansion of the medullary cavity, softening of the spongy bone, with thickening and infiltration of the periosteum and soft parts surrounding. The disease is confined to children and young adults. It is said that the first metatarsal is most frequently affected; next the first phalanx of the middle finger; then the thumb; rarely in some other part of the body. Caries without suppuration—"dry caries" so-called—occurs occasionally in the spine, and in that situation is not an uncommon condition; such phenomena are, however, manifested in other parts of the skeleton. Mr. Hutchinson* recorded a case of caries of the articular surfaces of the knee-joint, without suppuration; the specimen was subsequently examined by Mr. Eve, and typical tuberculosis found.†

Tubercular Periostitis. Periostum may be the seat of the primary tubercular disease. The close association of the periostum with the bone renders it extremely unlikely that the disease will progress to any extent in the former without leading to affection of the bone secondarily.

Acute inflammation of bone is extremely rare. It is a question for investigation whether some cases of acute epiphysitis occurring in young children may or may not be of tubercular origin.

Treatment. In considering the method of treatment, the question at once suggests itself as to whether it is possible for a cure to be effected without operative interference. It seems probable that the tubercular nodule at an early stage of its development may resolve, that is before there has been complete disintegration of the osseous tissue and a necrotic mass formed. Then, again, we are familiar with the existence of tubercular masses in the soft tissues which have lost their vitality, or which remain in a state of quiescence without giving rise to discomfort. Such cases of old tubercular disease are common in pulmonary tissue, and are frequently found in the lungs at a *post mortem* examination. Then, again, lymphatic glands after an active tubercular inflammation may remain without giving rise to discomfort, as a mass of inert and non-irritating tissue, for a whole lifetime. In the same way we believe that tubercular disease in bone may run a similar course. There can be no doubt, however, of the fact that although resolution is the best event that can possibly follow the tubercular process; the condition of quiescence without resolution is one of questionable security. There is a liability at any future time for the disease to light up afresh; this probably is accountable for those cases of hip-joint disease which recur after an apparent cure. Constitutional treatment is in all cases of great importance. This consists in good nourishing food, fresh air—sea air if possible—and exercise, combined with the administration of cod liver oil, iron, the syrup of the

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[*] *Transactions Pathological Society of London*, vol. xxxviii., p. 312, 1887.

[†] *Transactions Pathological Society of London*, vol. xxxix., p. 267, 1888.

hypophosphites, phosphorus, or some other equally good form of tonic.

In the early stage of the disease we may hope for resolution, and we must encourage this: the indications are, first, perfect rest for the diseased bone; this may be accomplished without interfering with exercise. Wherever we have to deal with tubercular disease, it is of very great importance that we interfere as little as possible with the free permission given to the patient to take all the exercise which the inclination dictates, avoiding exhaustion. If, however, exercise for any reason be incompatible with rest for the diseased bone, then the latter must always be considered the more important indication, and on no account must we allow anything to be a cause of unrest in the affected tissue in which we are hoping for the process of resolution. The method by which we can secure rest must vary according to the position and the extent of the disease. Macnamara[*] records a case of early tubercular disease in the os calcis treated with perfect success by the application of suitable apparatus. The beneficial effect of rest, combined with exercise, in caries, is exemplified in the use of the various forms of fixation apparatus used in joint affections, but a striking instance of it is seen in cases of tubercular caries in the spine. The treatment by the application of plaster of paris jackets, as introduced by Dr. Sayre, of New York, has yielded excellent results, and this method of treatment is now all but universally employed in spinal caries. The principles involved in the treatment are the same whether the jacket be made of plaster or poroplastic material. The method of applying the plaster of paris jacket so as to fix the spine in the position of extension, and thus prevent pressure of the diseased surfaces together, has, until recently, been by the use of suspension—the block-and-tackle apparatus being used after the manner with which, you are all familiar. The method suggested by Mr. R. Davy,[†] however, will be found quite efficacious, and probably more effectual in attaining the desired result. I refer to the method of slinging the patient in a hammock composed of some soft material, *e.g.*, cheese cloth. The patient lies on his face in the hammock, and the plaster is applied, the bandages being applied over the portion of the hammock lying in contact with the body. As the patient lies thus on his face in a hammock which is suspended at either end, there is very complete extension of the spine, and the plaster jacket secures it thus in the desired position. When the plaster has dried, the portion of the hammock above and below the jacket is cut away. When the cervical vertebræ are affected a different form of apparatus must be used. We combine with the jacket a jury-mast to support the head. I show you here a patient treated on these principles: (Fig. 1) instead of the ordinary jury-mast I have had substituted a croquet-hoop apparatus, first suggested by Dr. Elliott;[*] this is more convenient than the jury-mast, as it allows the patient to lie flat on her back without inconvenience, and is not as awkward as the ordinary apparatus. The patient wears a poro-plastic jacket, to which the head support is attached. On admission to the Children's Hospital she was complaining of a considerable amount of pain. After lying in bed, with extension applied to the head, for some weeks, the apparatus which you see was applied, and she was allowed to run about. She has been at the Home on the Island all summer, playing about in the sand; she has

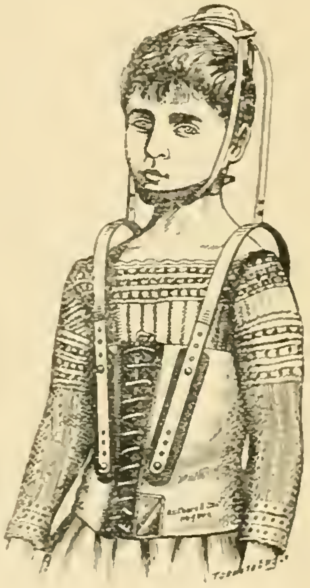

FIG. 1.

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[*] Macnamara: *Diseases of Bones and Joints*, p. 121.

[†] *British Medical Journal*, July, 1885.

[*] *British Medical Journal*, J[illegible] 1[illegible]4

had no pain nor discomfort since the jacket was applied, and her general health is excellent.*

When the long bones are affected, or the small bones of the foot or hand, fixation (suitable) must be provided, keeping the same principles of treatment by rest in view. When the disease is situated in such a position that counter-irritation may be combined with the treatment by rest, then the tincture of iodine painted over the part will be found serviceable. This will not be found of much value in spinal caries, possibly because the seat of the disease is too far removed from the surface. Indeed it seems just as legitimate to my mind, if the theory of reflex nervous action be accountable for the affect of the counter-irritation, to apply the counter-irritant to the flanks or the front of the belly as it is to apply it to the back, when the bodies of the dorsal vertebræ are affected, the nerve connections with the vertebral bodies being quite as intimate in the one case as in the other.

The operative treatment must now be briefly considered. In speaking of this I entirely exclude joint disease from my remarks, as this will be spoken of by the lecturers who follow me. The indications for operative interference are dependent to a great extent on the part of the skeleton affected. Spinal caries is seldom dealt with in this way unless there be an abscess developed in connection with it. First, then, we may consider that the formation of an abscess or the existence of a discharging sinus is always an indication for surgical interference. Then, again, where the disease is extensive, affecting perhaps a bone of the hand or foot in its entirety; where the disease is near a joint, and by its tendency to spread threatens to implicate the articulation, in these cases, too, the disease is oftentimes situated near the epiphyseal cartilage, and in young people the growth of the bone may be interfered with if the process of destruction at this point be not terminated; pain of great severity, is rarely the indication, but is to be considered in deciding upon a course of treatment in a given case. Lastly, I would mention that in certain cases where there are indications of pulmonary tuberculosis, far from this being a contra indication, I believe that if there be a primary affection of the bone it ought to be treated surgically and the disease removed. It may be that the diseased bone is a source of infection for the system generally and that a tubercular lung affection of recent origin may entirely disapppear if the tubercular disease of the bone be dealt with.

When an operation is undertaken the method of procedure must be decided on the merits of the individual case; the disease must be entirely removed, and the cavity left must be surrounded by healthy bone tissue and entirely free from any irritating discharge. An attempt must be made to set up a healthy action in the wound, with the hope that it will close by the development of healthy granulation tissues. Let me quote a case by way of illustration. A boy, æt. 11, was admitted into the Children's Hospital with extensive disease in the shaft of the right tibia and in the left internal malleolus. There was found a condition of caries in both situations with the existence of sinuses. All unhealthy granulations were scraped away. The bone was scraped by means of a Volkmann's spoon; in this way all the soft and unhealthy bone was removed, leaving a cavity with firm walls; this was thoroughly irrigated with perchloride of mercury 1-2000, and then stuffed with lint, soaked in chloride of zinc, 40 grs. to the ounce, and stuffing was left in for half an hour and then removed, after which the cavity was dusted with iodoform and was stuffed with lint, soaked in 1-2000 perchloride of mercury; an antiseptic pad was applied and secured by bandage; the left internal malleolus was treated in the same fashion. The object of such a method of treatment is, first of all, to remove the disease thoroughly, and then to leave healthy tissue behind. The effect of the chloride of zinc in strong solution is beneficial; it causes a microscopic sloughing of the surface of the tissue to which it is applied. The slough is formed apparently by a mixture of the chloride with the tissue; the slough remains as a thin film and protects the tissues beneath from further action of the solution, while at the same time it provides an excellent barrier against septic infection; the process of suppuration in a cavity so treated is entirely suspended for about forty-eight hours.

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*Since writing this lecture I have incorporated the croquet-hoop apparatus in a plaster jacket. This was done in the case of a patient two years of age, suffering from cervical caries. The result has been all that could be desired; the child has lost all pain, and, while she was previously fretful and irritable, she is now cheerful and bright, suffering no discomfort whatever.

Let me here caution you against the use of a solution of chloride of zinc of weak solution. This acts as an irritant in the tissues. Last summer I operated in a case of caries of the tarsal bones, and after removing the diseased bone I asked for a solution of chloride of zinc, 40 grs. to the oz., with which I stuffed the cavity. I discovered the following day that the solution was not 40 grs. to the oz., but very much weaker. The patient suffered extremely for some days after the operation, and I attributed it, whether rightly or wrongly, to the weak solution of chloride of zinc; it was not of sufficient strength to kill the tissue outright, but soaked into the tissues and acted as a strong irritant, setting up a degree of inflammation which continued despite all our efforts to get the cavity cleared out by irrigation. Finally, after ten days, during which the patient suffered great pain, I performed a second operation, scraped away all the irritating material from the walls of the cavity, and stuffed it with lint soaked in perchloride of mercury. Subsequently there was little pain, and the cavity entirely closed.

These cavities are slow to heal. The process of healing by granulation in an ulcer in the bone is of necessity very slow. Then, again, there is always a danger of these granulations becoming themselves the site of a tubercular process necessitating their removal by operation. All that surgery can do in these cases at present is, first of all, to remove the disease entirely; and, secondly, to treat the case subsequently in such a fashion as to secure, if possible, a healthy healing process. After operation a healthy action of the tissues is to be maintained by frequent dressing and irrigation with some non-irritating lotion, such as boracic acid.

What are the causes of failure in our treatment? Our interference surgically may set up such a degree of inflammation in the walls of the cavity, which are already the seat of condensing osteitis, that the effusion may cause pressure on the blood-vessels, and finally their obliteration, the result being that a necrosis takes place. It is of frequent occurrence in these cases to find thin lamellæ of bone cast off and coming away in the discharges, perhaps weeks after an operation. Then the granulations may, as I have stated, become the seat of a tubercular process; therefore it is that oftentimes all our best efforts to bring these cases to a successful termination fail, and finally we may have to resort to amputation of a limb, when the disease has become extensive, or the tubercular affection may have become general, and our patient may die of tubercular meningitis or phthisis pulmonalis.

The special efficacy of iodoform in treating the tubercular disease of bone is still a question of dispute; injections after the method of Verneuill are not favored by Dollinger, who found that in children they did not induce recovery in a single case; that injection of small amounts produce deafness, headache, and nausea; of larger quantities, loss of consciousness, impaired respiration, and acute cystitis. At the moment of injection some headache might be felt, and there might even be a rise of temperature three or four degrees F. The rapid evaporation of the ether may cause necrosis of the abscess wall, and if, for example, a psoas abscess should burst into the peritoneal cavity, death might result from such an injection. Iodoform oil, one in five, has been recommended for abscesses. It is said to cause a disappearance of the tubercle in the abscess wall. Of 109 cases treated one-third were apparently cured, and as many more improved; the small amount injected, 32 to 5[illegible] minims, will not cause iodoform poisoning.*

In concluding this paper, let me refer briefly to the treatment of cold abscesses formed in connection with tubercular disease of bone. The walls of the abscess cavity I have already described. The indications are to remove all the sloughs and gelatinous tissue, and to leave a wall of comparatively healthy tissue upon which granulation may spring up, which may eventually lead to a closure of the cavity. Formerly it has been the custom to scrape the abscess wall with a Volkmann's spoon; this, in addition to removing the sloughs, is apt to cause too much destruction of the tissue forming the wall. It causes, in fact, so much destruction and inflammation that further sloughing is apt to take place, and the formation of granulation tissue is postponed. The method recently suggested by Mr. Caird in the *Edinburgh Medical Journal*, of scraping the abscess walls with the finger-nail, is much preferable.† Here we are guided by the

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*Sajous: *Annual of the Medical Sciences*, 1890.

†*Edin. Med. Jour.*, September, 1[illegible], page 22[illegible]

sense of touch, and can readily feel when the wall is left smooth and free from shreds and sloughs. The abscess wall after such treatment readily takes on healthy action and the healing process is facilitated. Mr. Caird reports cases of a large gluteal abscess of two years' duration closed in three weeks, a double psoas abscess dependent upon Potts' disease healed in six weeks, and some other similar cases, successfully treated in this way. Dr. Cameron recently treated a tubercular abscess in the neck, of some years standing, in this way with excellent results. The abscess healed without any discharge of pus. These chronic abscesses are apt to become septic when opened. I use the term "septic" advisedly. In the chronic abscess we have the specific virus of tubercle occupying the field, but once give an opportunity for the entrance of those organisms, which are ordinarily accountable for septic changes in an open wound, and we have the whole aspect of affairs changed. We now have to fight a new enemy; the clinical phenomena formerly of a chronic nature now take on an acute action, and we then speak of the abscesses becoming septic, and the danger of general septic poisoning is greatly increased. In addition to the danger of becoming septic, these abscesses are liable to absorb materials thrown into the cavity, and therefore strong germicides must not be used because of the danger of producing toxic effects. The reason is that the walls of the chronic abscess are vascular and are not protected by granulation tissue. The lotion, whatever it may be, is readily absorbed, and if any sloughs or shreds of tissue line its walls, these take up the lotion like a sponge, and it is thus applied to the vascular walls beneath, and absorption takes place. Once granulation springs up the risk of absorption and septic infection diminish. Thus we recognize clinically the fact that chronic abscesses, when first opened, are very apt to become septic and to produce general septicæmia; but after a few days or weeks the granulations form a protective barrier, and the abscess, though open, is not so prone to become a source of danger. This explains the dread the older surgeons had of opening chronic abscesses, it was so frequently followed by septicæmia. Now, with antiseptic precautions, we can attack such collections without dread of such an unfortunate result.

Abscesses in connection with spinal caries forming a collection under the psoas fascia should be opened in the loin after Treves'* method. This affords one a better opportunity of keeping it aseptic than if it were opened in the thigh. A case in the Children's Hospital, in which a large collection was pointing in the thigh, I successfully reached by the lumbar incision. The case did well, and although an opening occurred spontaneously in the thigh two or three months after, still sufficient time had elapsed for the lining of the abscess wall with granulations to occur, and when the second opening formed the danger of septic infection had very much diminished.

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*Barker: *Manual of Surgical Operations*, page 111.

TUBERCULOSIS IN THE ENDS OF THE LONG BONES.

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Pathology.—The discovery of the bacillus tuberculosis by Koch in 1882 has placed the pathology of tubercle on a secure basis, and has enabled the profession to march to the attack of an enemy whose strength and tactics are known.

The word "tubercle" is common in the works of the old writers, but is used to signify any morbid product of small size and of globular or nodular form. The term referred to the form rather than to the nature of the morbid product, and the mass might be scrofulous, cancerous, syphilitic, etc. Since the time of Laënnec, the word has been used to signify that morbid something which gives rise to general phthisis. Nélaton, in 1836, made the first systematic statement in regard to bone tuberculosis, and gave descriptions of forms of the affection that stand well the tests of time and criticism.*

The usual seat of tubercular processes in bone is the spongy tissue. In order of frequency they occur in the vertebræ, the extremities of the long bones, the short bones of the hands and feet, and more rarely in the flat bones, and, exceptionally, in the shafts of the long bones, at a distance from the joints. Of the long bones, the tibia, femur, and humerus are the most common seats of attack, and in the order named. In the tibia the upper end is more frequently affected than the lower; in the femur, the lower end more frequently than the upper. This fact is of special interest in view of the mode of growth of these bones; the former grows more actively at the upper end and the latter at its lower extremity. Ollier has pointed out that fourteen-fifteenths of the growth in length of long bones occurs at the place of junction of the epiphysis with the diaphysis; and also that the terminal portions of the diaphyses, in children and adolescents, are most frequently the seat of acute and chronic inflammations and of various neoplasms. Tissues which are physiologically active are more fully supplied with blood than others, and are more liable to the attack of pathogenic processes. In keeping with these facts we find the knee more frequently the seat of disease than the hip or ankle, and the tibia more frequently the starting point than the femur. Another important clinical fact is also thus explained, viz., that bone diseases in the vicinity of joints are much more common in children than in adults, in whom the diaphyso-epiphyseal area has ceased to be active. When chronic disease affects the joints in the adult, it is much more frequently synovial in its origin.†

Vide Ashurst's International Surgery, vol. vi., p. 907.

†*Orthopædic Surgery*, Bradford and Lovett, p. 306.

*The common form of osseous tubercular infection is the focal or encysted, first described by Nélaton. On section of the diseased bone the first noticeable change consists of a local hyperæmia of the cancellous tissue, soon to be followed by a small, grayish, translucent spot, which increases in size, while, at the same time, the zone of hyperæmia extends, and the surrounding bone becomes boggy, or sodden, from the excess of fluid transuded. The deposits present two chief types: the main one is that of a distinctively degenerative ostitis, the osseous trabeculæ being almost entirely absent, and the deposit composed of a mass of cellular tissue containing tubercles usually undergoing caseation. There may, however, be a certain amount of formative activity, and a resulting sclerosis of the osseous trabeculæ with a mass of new bone of varying density, which constitutes a tubercular sequestrum.

The modes of termination may be arranged under four heads: (1) The absorption of the diseased focus is possible up even to a late stage, provided the disease is strictly local and no sequestra have formed. (2) The deposit may push its destructive work toward the surface and reach the periosteum outside the joint, and give rise to a peri-articular abscess. (3) The diseased part may break down into pus, which may become cheesy or calcified, and remain quiescent for an indefinite length of time. (4) The fourth mode of termination—probably the most common—is also the most destructive; it reaches the articular cartilage and breaks into the joint cavity, setting up a purulent arthritis, or what is probably more common, causing an exacerbation where an arthritis had already been present. It is quite certain that the joint proper often becomes inflamed before any direct communication has been established with the diseased focus within the bone.

Lannelongue, in an early autopsy in a case of hip-disease, found a focus the size of a pea in the epiphysis, two millimetres from the cartilage. The synovial membrane was reddened and fungous; the capsule thickened; and the round ligament vascular and softened, although there was no effusion and no connection with the focus of disease in the epiphysis. Volkman has found even more pronounced joint changes and swelling of the peri-articular structures before pus had entered the joint.† The extension to the joint in the manner indicated is in some measure dependent upon the relation of the synovial membrane to the line of epiphyseal junction. In the shoulder, ankle, and wrist, the disease remains a long time isolated, or gives rise to peri-articular infiltration and suppuration, while in the case of the hip, where the epiphysis and the upper end of the diaphysis lie within the joint, the destructive process, with greater frequency, becomes intra-articular.

Diagnosis.—There are no symptoms peculiar to tubercular affections of the bones—no pathognomonic signs. By consideration, however, of all the conditions of the patient, one may arrive at a strong presumption. A few symptoms and signs may be referred to as pointing very strongly to the existence of tuberculous infection of the bone in close proximity to a joint. In the domain of surgery, early diagnosis is never more urgently called for than when trouble threatens near a joint. It is not necessary to discuss cases that are far advanced; the evidence in them is so plain that an error can scarcely be made. If the products of disease can be obtained, bacilli may be found and a diagnosis established. Among the most reliable indications of incipient tubercular disease in the immediate vicinity of a joint are:

(1) *The history, family and personal.*—Concerning the former, nothing more need be said than that it is just as important as it is when chronic disease of the lungs is suspected. If the personal history is enquired into, it will, in a very large number of cases, be found that the onset of the affection has been preceded by some disease which has greatly lowered the general tone. It will often be found that the patient had recently suffered from whooping cough, scarlet fever, summer diarrhœa, typhoid fever, or other exhausting disease, and had made a poor recovery. It is doubtful whether the preceding sickness has any other relation to the tuberculosis following than that it simply lowers the vital powers and renders the tissues more vulnerable than they otherwise would be. Under such circumstances a predisposition, previously kept in check, is able to manifest itself.

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*Cheyne: *Lancet*, Nov. 15, 1890. *Orthopædic Surgery*, Bradford and Lovett, p. 215.

†Bradford and Lovett, p. 217.

A similar result follows from unhygienic surroundings.

(2) *Lameness of an uncertain commencement.*—A slight stiffness of the joint in the morning, with a scarcely noticeable limp, and a history that is indefinite as to the time when the limp was first manifested, is a valuable indication.

(3) *Some rise of temperature, especially in the evening, both local and general.*—Too little attention has been paid to this symptom. From observations made by Bradford and Lovett, of Boston, it is manifest that it is an early and valuable indication of incipient disease.

(4) *Limitation of the functions of the joint nearest the disease.*—This is, no doubt, the result of reflex action and manifested long before the joint has become infected by direct communication with the tubercular focus.

(5) *Atrophy of the muscles which control the neighboring joint.*—This is a sign that occurs so early and in so marked a degree that it cannot be accounted for by disuse only. There is manifestly a direct trophic disturbance as well. By careful comparison of the two limbs this will be found a very early and very constant indication of disease in the joint neighborhood.

(6) *Early enlargement of the bone.*—Where the deposit is at all considerable it causes a perceptible inflammatory thickening of the bone, and careful observation will reveal a local enlargement that will be all the more manifest because of the adjoining muscular wasting. By careful measurements made from day to day, and comparison being made with the corresponding part on the other side, very slightly increasing enlargement may be observed. There will probably be some tenderness also, and a less resonant note upon percussion over the affected area of bone.

(7) *Flexion of the joint.*—In slight degree unobservable except by the most careful comparison, this sign is generally manifested at the hip; knee, and elbow. There is one special feature of great importance in arriving at a diagnosis in suspected cases—the element of chronicity. When the onset of disease is sudden and definite one may well conclude that it is not tubercular; but when there is a history of disability of a limb of uncertain beginning, particularly if accompanied by an account of exacerbations, sometimes apparently nearly recovering, and most of the evidences of disease just named are found present, the clinical picture may be considered complete.

Treatment.—The frequency of tubercular affections is something appalling, and though the virulence of tuberculosis has made it one of the greatest scourges of humanity, yet the more complete knowledge of its natural history, its pathology and terminations, and the influences which modify its course, have done much to bring it under the control of science. At least one person of every seven dies of some form of this disease. Its ravages are seen not only in its death-roll, but in disfiguring scars, crooked spines, and stiff and deformed joints. While pathology has been showing the natural course of the disease, surgery has been making advances which enable us to utilize the added knowledge.

Though the bacillus is the essential cause of tuberculosis, yet the element of fitness of soil is one of great importance. No doubt many persons, from birth, possess a degree of resistance to infection that would insure their safety even against direct inoculation. On the other hand, there are certain conditions that may be considered as promoting the fitness of the tissues for the development of the bacillus. Chief among these may be named *inheritance, defective hygienic surroundings, poor food*, and *age*. Bone tuberculosis manifests itself especially between the ages of three and nine years, and its time of greatest frequency of occurrence corresponds with the period of greatest activity at the places of growth in the long bones.

The medical treatment of those suffering from osteo-tuberculosis must be tonic and supporting. As in other forms of tuberculosis, bitter tonics, arsenic, quinine, iron, and cod liver oil, must be given. Of these, the last is most to be depended upon. Of much greater importance, however, is the dietetic and hygienic treatment. The most potent agents for good are found in sunlight, fresh air, and physiological rest.

Case 1. T.H., male, æt. 32; good family history: of good habits: had always been healthy till eighteen months previous. At that time he fell into a lake, and after removal was resuscitated with difficulty. Did not regain his usual health, and twelve months afterward felt pain about left hip, groin, and thigh. Was soon confined to bed: had much pain in thigh and knee. Soon pus formed, and openings were made in the perineum and on the outer side of thigh below and behind the great trochanter. Bare bone is now (March 15, 1890) found at the upper part of shaft of femur and the pelvic bones. Measuring from the ant sup. sp. process there is two inches of shortening; suppuration is abundant; there is great emaciation, waxy color, and râles at the back of lungs from hypostatic congestion; 45° of flexion.

Fixation of the limb by the Thomas' posterior splint; ordered to be carried out and kept out of doors every fine day, and given emulsion of cod liver oil and ext. malt. With little modification this treatment was continued. July 17th patient wrote: "My sores have quit running; my leg is healed up; there is not much pain. I have gained more than forty pounds in two and a half months." I have not examined this patient since the splint was applied.

Tuberculosis is a self-limited disease as much as smallpox or scarlet fever. The bacillus is short-lived and perishable, instead of being tenacious of life. If nature can be so supported as to bear up till the bacillus has lived out its short span of existence, and if strength can be imparted to repair the ravages committed by the enemy, then the once tuberculous patient may again enjoy robust health. As witnesses to this fact we may point to many who for years were invalids because of the invasion of this disease, who have survived its virulence, and who, though in most cases still bearing the marks of the conflict, yet are to-day active and useful members of society.

In the matter of treatment the army of surgeons have separated themselves into two hostile camps. They have been called by John Croft, of St. Thomas' Hospital, the *excisionists* and *non-excisionists.* While discussion from these extremes may do much to throw light upon this subject, yet to moderate men the divorce thus effected must be viewed as disastrous in practice. Neither party is in possession of "the truth, the whole truth, and nothing but the truth."

The surgeon who would never resort to operative measures would lose much valuable time, and could not be credited with true conservatism. Foci in the vicinity of a joint are a great menace to its safety: and timely evacuation by operative measures is truly conservative. As the diagnosis of a single central cheesy focus is not easy to make, if its presence be suspected in the vicinity of a joint, exploration by a probatory incision is justifiable.

While much may be said for operative treatment, yet it must ever remain a fact that the great majority of these cases will come under the notice, not of the well-equipped hospital surgeon who is prepared to carry out, with thorough asepsis, the latest and most approved operative measures, but of the busy practitioner who deals with all kinds of cases in his daily round. Hence methods of treatment which can be adopted without resort to heroic measures are of much greater general interest and value to the profession. In using the terms *operative* and *non-operative* treatment, if one were bound to admit that non-operative treatment meant neglect, and that its results were less successful than those following operation, then he would be in error in advocating the palliative as against the more active treatment. In speaking of the treatment of tuberculous joint affections, Croft, who himself advocates operation, says: "After all deductions have been made on both sides, it must be admitted that, up to the present time, the rate of mortality is slightly in favor of the non-excisionists."*

Howard Marsh, of London, claims to have more than ninety *per cent.* of cures of cases of hip disease, and to get better results, *i.e.*, less shortening, less deformity, etc., than are obtained by operative procedures.

The non-operative treatment of tuberculous affections of joints or of the bone in the immediate vicinity may be summed up under two heads:

1. *General treatment.*—This comprises the treatment by drugs, before referred to, and general dietetic and hygienic care.

2. *Physiological rest.*—The importance of securing rest for the inflamed part is as important here as it is in the management of a case of pleurisy, or of conjunctivitis. In a tubercular focus the process of repair often meets with great obstacles. The embryonal cells, of low vitality from the first, become the victims of the ptomaines of the bacillus, and hence only under favoring circumstances, provided by the surgeon, can they develop into tissue of a higher type.

**Lancet*, February 8th, 1890.

A prime requisite is to secure for the part a favorable condition of the circulation, which can be done only by keeping the part at rest. In order to carry out this indication efficiently, it may be necessary to keep the patient in bed. As these cases, however, are noted for their chronicity, and months or years are likely to elapse while under treatment, the confinement indoors entails the non-use of the most efficient agents at our command for sustaining the patient's vital powers. The dietetic care of the case may be most thorough, but it will not avail if, through confinement and inactivity, the powers of assimilation give out. Cases that have shown a pallor and general want of tone, that marks the plant that is kept from the sunshine, will often improve rapidly when taken from bed and permitted to live a great part of the time in the open air.

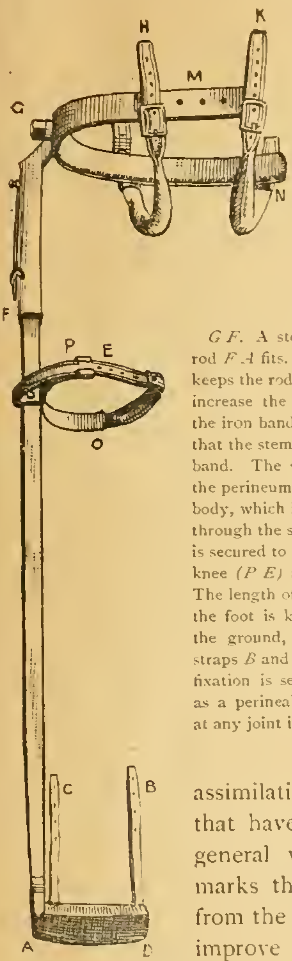

FIG. I.

$G F$. A steel box into which the steel rod $F A$ fits. At F is a spring stop that keeps the rod in place, and enables one to increase the length from G to A. At G the iron band $N G M$ is fixed by a bolt so that the stem $G A$ does not move upon the band. The straps H and K pass under the perineum and carry the weight of the body, which is transmitted to the ground through the stem $G A$. The patient's leg is secured to the stem by a strap above the knee *(P E)* and another above the ankle. The length of the stem is so adjusted that the foot is kept about three inches from the ground, and extension is made by straps B and C. Thus extension is made, fixation is secured, and the splint, acting as a perineal crutch, prevents concussion at any joint in the limb.

Case 2. J.K., male. 17 years. At Christmas, 1886, had pain in sacro-iliac region, which afterwards extended to the front of body. Was confined to bed for some weeks. In May, 1887, a spinal support was applied for Pott's disease. At Christmas, 1887, sat down after skating, and on rising was unable to remove his skates. Was confined to bed, the right leg being painful and stiff. In March leg was swollen and flexed to nearly a right angle. Extension by means of a weight and pulley was now employed, and the flexion corrected. Was in bed five months; no discharge. Was in hospital September to December, 1888; extension by weight and pulley was employed. Abscess formed, and an opening was made on outer side of the thigh.

July 14th, 1889. Is pale, but fairly well nourished; the leg is swollen and tender throughout its whole extent. Is moved with much difficulty because of the tenderness: suppuration continues from sinus at outer side of thigh. No flexion, no shortening, no movement at the hip.

Aug. 6th, 1889. Applied a long splint, which afforded extension and fixation and acted as a perineal crutch, and had the patient out of doors as much as possible. Improvement was immediate and rapid (*vide* fig 1).

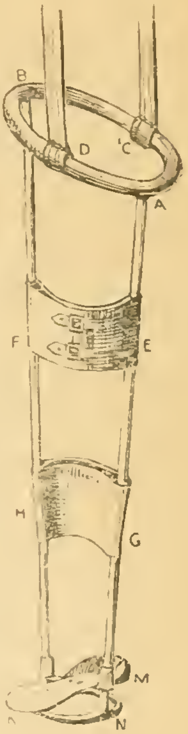

FIG. II.

$A N$ and $A K$ are steel rods. $A B$ is an iron ring, well padded and covered, and fixed so that its plane makes an angle of 125 with rod $A N$. The ring is shaped accurately to the shape of the upper part of the thigh. $H G$ is a broad strap behind calf of leg, and $F E$ one that passes in front of the lower end of the femur. $K N$ is a foot-piece that moves up and down upon the rods, and serves to keep the toes from touching the ground. The length of rods is so adjusted that the foot comes two or three inches above the ground, and the splint becomes a perineal crutch.

Oct. 11th. No discomfort in walking with splint, leg handled freely: discharge has ceased, appetite good, sleeps well.

Improvement has been uninterrupted, except for the appearance of a cold abscess in the dorso-lumbar region. Since the spring of 1890 he has been attending regularly to his duties as book-keeper, and is still wearing the splint.

By the employment of some form of portable fixation apparatus this end may be achieved. If the tubercular focus be in the foot or in the vicinity of the knee, the part may be kept at rest and in comfort by encasing it in a gypsum bandage, while the patient is permitted to move about either with crutches, or, better, protected by a Thomas' knee-splint, which is equally effectual for the protection of the foot (fig. 2). If the disease be in the vicinity of the hip-joint, the American traction-splint (case 2 and fig. 1) affords rest to the part and at the same time acts as a perineal crutch, permitting movement without much inconvenience. Patients wearing these may go to school or to business, and find themselves but little hampered in performing the ordinary duties of life. More complete fixation of the hip-joint and more effectual rest for the diseased part may be obtained by the employment of a modification of the Thomas' hip-splint, by which traction is, at the same time, employed.

In disease at the hand, elbow, or shoulder, the management is more effective and simpler. The hand or wrist may be encased in gypsum. The elbow may best be kept at rest by carrying the hand up to the neck and securing it there by a roller passed around the wrist and neck alternately; in such a manner as to hold them in close contact, but not to permit the bandage to so tighten upon either as to interfere with the circulation. This is a very simple and most efficient means of immobilizing the elbow, either in disease or after reduction of dislocation, and was first employed by Hugh Owen Thomas (*vide* fig. 3).

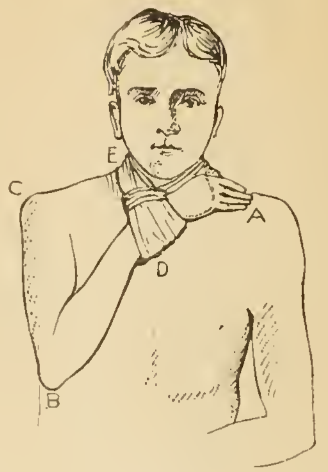

FIG. III.

The wrist is to be brought up well against the neck, and so secured by bandage that no undue constriction will occur. The elbow will now be found to be thoroughly immobilized.

The same bandage, together with a well-moulded cap over the shoulder, may be employed for keeping this joint at rest. Even when it is necessary to keep the patient entirely at rest upon a bed, it is better that the bed or frame employed should be readily movable, so that the patient may be carried out of doors, and kept in a tent or in the shade of a tree, if the direct rays of the sun are too strong.

Even when the patient must be confined to the bed and to the house, as must be done sometimes, in winter, it is better to employ a splint to secure fixation and traction than to use the weight and pulley. The former is attached only to the patient and moves when he moves; the latter is attached to the bed as well as to the patient, and movement may be effected only at the risk of disturbing the limb.

When it has been found necessary to operate for the removal of the diseased area, fixation should be maintained for a long time. It is necessary not only to assist in guarding against the return of the disease, but to prevent deformity.

Up till the present time the treatment of joint diseases due to tuberculosis by non-operative measures has given better results than treatment by excision of the joint. The rate of mortality is better, the functions of limb are better preserved, the dangers of general infection are not so great, and the time required to accomplish a cure is probably not much longer. Under the plan of treatment by a portative apparatus the element of time is not one of great consequence, because the child may frequently go to school, and the adult attend to business for years, while under treatment. Parents acquiesce more readily in a plan that does not involve the use of the knife; and it is one that is at the command of the general practitioner.

When the presence of pus is demonstrated, and it is accompanied by declining health, operative measures should be employed to give it exit, and at the same time diseased tissues may be removed. When a tuberculous focus is so situated that it may be removed without opening a joint, it should be done. Undoubtedly there is a large field for the wise employment of operative measures; but, in the main, Nature is a wise mother, and any aid proffered her should be discreet. There are two indications upon which we may confidently rely, in the fulfilment of which we certainly strengthen her to do the best for her afflicted children—give the patient *good food, good air, and sunlight*, and give the diseased member *rest*.

SURGICAL TUBERCULOSIS.

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IT is almost unnecessary to insist in these days on the absotute identity of strumous or scrofulous affections and tuberculosis. Their unity is sufficiently attested by the following facts: Wounds accidentally or experimentally inoculated with tuberculous matter have subsequently become the seat of affections hitherto characterized as "strumous." In the second place it is found that all those diseases known as strumous, show, under the microscope, both tubercles and tuberculous tissue. Thirdly, these affections are shown to be tuberculous in nature by the constant presence in them of the tubercle bacillus. The detection of the bacilli in these tissues is, however, by no means always easy, and one may examine many sections from such a tissue without encountering a single bacillus. It is more than probable, however, that they are not so few as this would make it appear, for they may be in the spore-forming stage, and so not take the stain, or, as Watson Cheyne suggests, they may fail to respond to the ordinary stain-tests at various stages of their growth. Moreover, inoculation of such tissue into the anterior chamber of a rabbit's eye, seldom fails to produce typical tubercles on the iris, and quite recently Mr. Watson Cheyne has performed a very extensive series of inoculation experiments upon rabbits, guinea-pigs, and goats, which have demonstrated, beyond cavil, that precisely similar results follow the injections, whether the injected material be caseous matter from a strumous joint or abscess, or an emulsion of a pure culture of tubercle bacillus.

From a histological standpoint the tubercular process is recognized as manifesting itself in two ways; first, in the form of the well-known "tubercle" or "giant-cell system," and secondly, in the less classic, though certainly not less characteristic, "tuberculous tissue," which consists in an infiltration of epithelioid cells. These cells are described by Cheyne as running through the tissues in broad bands, or being found at irregular intervals among the other tissue elements. When giant-cells are present they usually contain bacilli, but bacilli are also found amongst or in the epithelioid cells, often clinging close to the nucleus. On the other hand, they are never found in the inflammatory tissue surrounding the tubercle, unless they have been accidently washed into that position during the process of preparation.

The most potential factors in the causation of the disease are (1) a peculiar vulnerability of tissue, and (2) the introduction through some channel of the tubercle bacilli. That a peculiar vulnerability of tissue to the ravages of the tubercle bacillus is hereditarily transmitted from generation to generation cannot be denied, and the excessive physiological activity present in the neighborhood of growing bone no doubt acts as the determining cause in the lodgment and growth of the bacilli near ossific centres or epiphyseal cartilages. Having once lodged and asserted its baneful influence, the further course of the disease is largely determined by the varying conditions of rest or irritation, and by sanitary or unsanitary environments as regards clothing, food, and ventilation.

Being in possession of these facts we direct our treatment accordingly, and thus it is that attention to constitutional conditions takes precedence of all others. It is impossible to exaggerate the importance of attention to hygienic details in the treatment of tuberculous diseases. Numerous cases are on record in which, under unusually good surroundings, the system has been able successfully to combat even advanced local disease of this nature. Nourishing, easily digestible food, in sufficient quantities and at appropriate intervals, is of the first importance. Scarcely second in this regard must be ranked warm, clean, dry clothing, and abundance of warm, fresh air, both by day and by night. Too often patients who are carefully carried out into the open air during the day are required to sleep in a close, ill-ventilated room, possibly with other children in various conditions of health or disease. Such inconsistencies must be carefully avoided. The most important internal remedy is cod liver oil, and this must be regarded rather as a food than a medicine. As much of this important aliment must be given as the digestive system of the patient will dispose of, and its various preparations should be tried until one is found that agrees well with the patient. A most useful adjunct to constitutional treatment is the application of appliances by which absolute rest to the part is insured. It is not sufficient to put the part physically at rest; it must be given physiological rest as well; it must not be permitted to perform any function. Where this is at all possible, such rest should be obtained for the diseased part, while at the same time all other parts of the body are allowed free and unrestrained exercise of all their functions, so that the patient may move about in the open air. In order to accomplish this the surgeon's ingenuity is sometimes severely taxed, and in some cases, from the nature of the parts diseased, it is impossible.

In disease of the spine various appliances are used with satisfaction. For ordinary cases in which the symptoms are not very acute, nothing is better than a well applied plaster of paris jacket. As the case advances towards recovery this may advantageously be exchanged for a poroplastic jacket, which has the advantage of being lighter and more easily removed for cleansing purposes. In the acute stages, however, a double Thomas' hip-splint, with an extension upwards upon which the occiput may rest, is a very useful apparatus. It is much less uncomfortable than would at first appear, and the patient can be carried about upon this with the greatest ease and without causing any movement of the diseased parts. The occipital extension renders it suitable for cases in which the disease is in the upper dorsal or cervical region, as long as the patient is in the recumbent position; but in these cases, as soon as the acute symptoms have subsided, the jury-mast or croquet-hoop apparatus should be substituted for it, in order to allow the patient to walk about. For disease of the hip I know of no better apparatus than Thomas' hip-splint. It may be made double where both hips are affected, or where the child is so young that it is impossible to secure a single splint firmly. This splint may be combined with ordinary extension apparatus during the acute stage while the patient is confined to bed, but when he is up the weight of the limb usually produces sufficient extension—the opposite boot being provided with a patten and the patient using crutches. Knee disease, again, is well treated by Thomas' knee-splint, and with this apparatus and a patten under the sound side, the patient can walk well without crutches, and at the same time keep the knee absolutely at rest—physically and physiologically.

Disease of the tarsus, ankle joint, or lower end of the tibia, may be satisfactorily treated by fixing the part in plaster of paris and providing the patient with a peg-leg, into which the bent knee is fitted; or the same splint may be used as in treating the knee. Tuberculoses of the upper extremity are more easily treated, as the parts may be placed at rest without difficulty without confining the patient to bed.

The question of the length of time such treatment should be continued before resorting to operation has been the subject of many discussions. Lister, Cheyne, McEwen, and Barker may be mentioned among those who advocate early operation, and over against them may be placed Howard, Marsh, Bowlby, and others. Without entering upon a discussion of the points for and against early operation, I am inclined to give my adherence to the views of McEwen, Horsley, and others, who operate, if possible, *before* caseation has set in. The results obtained by this method of procedure are most encouraging and in some instances brilliant. When one considers the almost absolute certainty with which a wound may be defended from the attacks of pus-forming organisms in these days of asepsis and antisepsis, it certainly seems justifiable to try to thoroughly eradicate from the system a local focus of disease which momentarily threatens to become general. We have all seen cases in which, as a result of operation, a patient has been almost immediately relieved of a diseased focus, which, under the more considerative treatment of rest, hygiene,and opening abscesses, would have kept the subject an invalid for months and perhaps years. We must not forget, however, that relapses sometimes follow operations, nor must we ignore the fact that an operation may remove such an amount of tissue that considerable disability may result either immediately or remotely.

On the other hand, those who advocate a more considerative line of treatment, simply opening abscesses as they occur and attending to the hygienic welfare of their patients, are able to show some excellent results, both as regards low mortality and the usefulness of the diseased member.

An operation having been decided upon in any given case, the plan almost universally adopted, where it is admissible, is that known as Esmarch's bloodless method. The limb is exsanguinated, either by the application of an elastic bandage or by elevation—preferably the latter—and a tourniquet is applied above the seat of operation. All hemorrhage being thus adequately prevented, the surgeon can make a deliberate dissection of the parts, his vision is not obscured by bleeding, and the healthy can be easily distinguished from the diseased tissues. In removing the diseased tissues, Barker uses what he calls a flushing scoop, by which a constant stream of hot water is kept flowing through the wound, thus washing away the diseased tissue as soon as it is loosened. Every particle of diseased tissue should be removed, and the external openings should be so free as to bring all parts into the view of the surgeon. The after treatment varies with different surgeons. Barker fills the wound with an emulsion of iodoform in glycerine (10 per cent.), then squeezes this out and closes the wound with stitches without any provision for drainage, applying the dressings in such a way that the least pressure will be over the line of the incision, so that any excessive secretion will be squeezed out between the sutures. Lister and Watson Cheyne, after thoroughly clipping away all diseased soft parts and gouging out all the diseased foci in bone, swab the whole wound area with very powerful germicides, such as zinc. sulph. gr. xl. ad. ℥j, hydrarg. perchlor. 1-500, or ac. carbolic, 95 per cent. They hold that even the pure carbolic does not prevent union by first intention. In addition, they always use iodoform in some shape. The disinfection of the wound having been completed, thick absorbent antiseptic dressings are applied and bandaged with considerable pressure, so as to limit capillary oozing. The tourniquet is then removed and the part elevated. In this way the interstices of the wound become filled with blood-clot, which becomes organized, and this conduces to the rapidity of healing.

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